FIVE THOUSAND AMERICAN FAMILIES—

PATTERNS OF ECONOMIC PROGRESS

VOLUME VIII

Analyses of the First Eleven Years of the Panel Study of Income Dynamics

Edited by Greg J. Duncan and James N. Morgan

With Contributions by Richard D. Coe, Mary Corcoran,
Linda Datcher, Greg J. Duncan, Daniel H. Hill, Martha S. Hill,
James N. Morgan, and Thomas Neubig

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INTRODUCTION TO VOLUME VIII

The Panel Study of Income Dynamics continues to collect data annually from a large, representative sample of families. These data have been analyzed in past volumes in this series as well as by researchers outside of the Survey Research Center. Although the content of the annual interviews is much the same from year to year, it has been possible for us to insert new questions from time to time. Several chapters in this volume analyze questions unique to the eleventh (1978) interviewing wave. Past work on the structure of wage differences between the races and sexes has suggested the importance of access to information and influence networks in early career jobs. The analysis of the use of these networks by Linda Datcher, Mary Corcoran, and Greg Duncan in Chapter 1 shows that most workers heard about their first jobs with their current employers informally and more than one-third reported receiving help in getting the jobs. Women were less likely to report this than men, while contrary to our expectations, black men were more likely than white men to report informal information and assistance. The data failed to show pervasive effects of informal channels on wages, however.

Eleven years of information on the unemployment experiences of panel members provides a look at the distribution of long-term unemployment and the links between past and future unemployment episodes. Mary Corcoran and Martha Hill address these issues in the second chapter and have found that the burden of long-run unemployment is shouldered by a very small fraction of workers. Although past unemployment episodes are correlated with future unemployment, no evidence of a causal link is found. That is, past unemployment as such does not seem to leave devastating scars which make future unemployment more likely. Rather, it appears as though unmeasured characteristics of the unemployed cause the correlation to appear.

The extensive set of retirement questions included in the eleventh wave questionnaire, supplemented with year-by-year reports of labor force status, provide the data for James N. Morgan's analysis of "Retirement in Prospect and Retrospect." Morgan has found that retirement decisions are dominated by health and the adequacy of retirement income. He found little support for the traditional economic model of income and substitution effects. A better model, Morgan believes, is one with retirement income goals set by past consumption levels and current economic conditions. Reports of the retired are consonant with retirement expectations of workers, although the importance of health is naturally greater in retrospect than in prospect. Finally, our data confirm results of many other studies that retirement is desired and enjoyed, except when it begins unexpectedly--usually because of health problems.

The concept of "occupation" has many wage-related dimensions, one of the most important of which is the authority exercised over subordinates. In the fourth chapter, Martha Hill analyzes responses to questions concerning the extent and nature of authority at work. Not surprisingly, she has found that men are much more likely than women to have say over the pay and promotion of others. Of greater interest is her finding that educational attainment and past work experience has a much stronger effect on a man's chances of being in a position of authority than on a woman's. Generally, the results provide some support for an institutional view of how labor markets operate.

The Panel Study follows new families formed by children in the original families who "split off." The data available for these splitoffs is unique in that information reported by them can be matched with parental information reported by the parents themselves. In the fifth chapter, Linda Datcher uses this information plus data on the community to test a model of early status attainment. Particular emphasis is placed on quantifying the effects of discrimination against blacks that operates from the community. She has found that the community measures are indeed important predictors of economic status, suggesting that community differences are as important as family differences in affecting status.

The encouragement of the Social Science Research Council's Committee on Life Course Perspectives on Middle and Old Age prompted the analysis of life events presented in Chapter 6 by Greg J. Duncan and James N. Morgan. The eleven years of data provide counts of the frequency of a host of major life events, ranging from demographic (marriage, divorce, becoming widowed, leaving the parental home) to economic (voluntary and involuntary job and residential changes, major changes in work hours and incomes) to health-related events. It is found that such events are surprisingly frequent, although generally concentrated in the younger years. Few panel members escape undesirable events completely, although educational attainment and assets do provide some protection. The analysis also suggests that events such as disability and the addition of older relatives to the household have detrimental effects on feelings of efficacy.

Data on splitoffs has also been used by Mary Corcoran in the seventh chapter to compare measurement error in reports of parental background between men and women. For splitoff children of both sexes, reports of parental status can be compared with actual reports by the parents themselves. Corcoran has found that daughters are considerably more reliable than sons in reporting parental attainments, especially in reporting on mothers' attainments. These findings, combined with those reported by Corcoran in Volume VII demonstrate the importance of addressing measurement error in models of status attainment.

In the eighth chapter, James N. Morgan describes the incidence and consequences of disability among kinds of households. Severe disability is quite rare in the population as a whole, although nearly one-fifth of all household heads report disability that is serious enough to limit the amount or type of work they can do. Disability has a substantial effect on labor force participation, but little effect on the earnings of those who continue to work. Estimating the economic impact by comparison with those without any disability provides slightly larger estimates than simply entering disability status as another variable. There is a substantially greater impact of disability on blacks, so that programs to aid the disabled are doubly redistributional.

The economic plight of many elderly homeowners is aggravated by the fact that many are unable to consume the equity they have built up in their homes without selling them. In the ninth chapter, Thomas Neubig demonstrates the potential benefits of Reverse Annuity Mortgages--a scheme which provides homeowners with a lifetime annuity using the house equity as collateral. He first shows that, contrary to what many believe, house prices have increased almost as rapidly among the elderly as among younger homeowners. He then demonstrates that Reverse Annuity Mortgage payments would lift a substantial number of elderly homeowners above the poverty line.

Recent rapid increases in energy costs pose two important policy-related questions: how responsive are consumers to these price increases and how is the burden of higher prices distributed across income classes. In Chapter 10, Richard Coe addresses these questions as they relate to utility payments, while in Chapter 11 Daniel Hill investigates these same questions for gasoline expenditures. Coe has found that low income homeowners--especially the poor, elderly, and blacks--have been particularly hard hit by utility price increases. Nearly one-third of the families in the lowest income decile spent at least one-fifth of their incomes on utility payments. He has also found some response to the higher utility prices. For example, each 10 percent increase in utility prices increases utility expenditures by about 5 percent.

The Panel Study questionnaire gathers annual data on the miles driven by the household members in the sample. When combined with assumptions about fuel consumption and gasoline prices, the data show both the distribution of the burden of high gasoline prices and the short- and longer-run responses to these burdens. As in previous analyses, Daniel Hill has again found that gasoline expenditures are income elastic, although a small, identifiable group of low income households with no access to public transportation spends a large fraction of its income on gasoline. In addition, he has found that families have adapted to higher gasoline prices. Over a five-year period, a one percent increase in gasoline prices was found to decrease miles driven by one percent. For movers, however, the decrease was twice as large.

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Chapter 1

INFORMATION AND INFLUENCE NETWORKS IN LABOR MARKETS

Mary Corcoran, Linda Datcher, and Greg J. Duncan

INTRODUCTION

Most of our knowledge about how labor markets operate has been based on cross-sectional data which provide a snapshot picture of the working population at a point in time. When cross-sectional data are used to estimate the path of earnings over time for different labor market subgroups, it is usually found that earnings rise more rapidly for white men than for other groups, suggesting that the *initial* wage gaps between white men and other groups begin small and grow larger over time.[1] Recent analysis of data from the Panel Study of Income Dynamics has cast considerable doubt on this finding. Hoffman (1978) used the longitudinal nature of the data to show that the earnings of black and white men in their thirties grew at roughly equal average rates between 1967 and 1974. Growth rates in earnings among males in their twenties did differ by race, however, and were twice as high for whites as for blacks. Duncan and Hoffman (1978) and Corcoran (1978) showed that the apparent differential payoff to years of work experience between men and women in the cross-section resulted from a failure to adequately specify the *components* of work experience. When years of total work experience was decomposed into segments measuring years prior to working for the current employer, years prior to taking the current position, years of training associated with the current position, and years of post-training tenure, it was found that the estimated wage benefits associated with additional years of a particular type of work experience was similar for workers in different race and sex subgroups. The value of additional years of the various types of experience differed considerably, however. The payoff to training years was the highest, followed by years of experience with the current employer prior to taking the current position. Different apparent wage returns

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[1] See, for example, Hall (1973), pp. 393-94, or Corcoran (1978), pp. 78.

to total work experience resulted, in part, from different average amounts of time spent in the various work experience segments.

But race and sex differences in work history accounted for less than one-third of the earnings gap between white men and the other race/sex subgroups. An extensive search for other productivity-related characteristics such as absenteeism, desire to take jobs with locations or hours that were compatible with household responsibilities and plans to quit work also failed to explain very much of the wage gap. A large, unexplained wage differential remained. It was substantial in the early years of labor market experience and increased somewhat over time.

To help explain those early wage differences, a series of questions about job search and acquisition was included in the eleventh (1978) interviewing wave of the Panel Study of Income Dynamics. In particular, we asked all household heads under 45 years of age how they heard about and obtained their first jobs with their current employers and their first career jobs. In the case of working couples, we also asked the husband if any information and influence process had helped his wife in obtaining her first job with her current employer. With these questions, we sought to extend our current knowledge about the role of information in the job acquisition process to include the influence process as well. It is one thing merely to hear about a job opening from a friend or relative and quite another to actually know someone who works there or to receive help in getting the job itself. If the influence process favors white men relative to the other groups, then credible explanations of the process of discrimination could be constructed.

In the first part of this chapter we review briefly the relevant literature on information and influence and provide a theoretical outline of the role they play. We then describe how information and influence networks are used by various types of workers and estimate the resulting wage effects. Our findings are summarized at the end of this chapter.

LITERATURE REVIEW

Much of our current empirical knowledge of labor market information networks comes from a study of the Chicago labor market conducted in the early 1960s by Rees, Schultz, Ullman, and others.[2] Four white-collar and eight blue-collar occupations were studied. The single most important finding was the pervasive use of informal channels of information, especially for blue-collar jobs. Informal sources such as referrals from employees or other employers accounted for about half of all white collar hires and four-fifths of all blue collar hires.[3] Access to these informal channels was found to differ substantially between blacks and whites. The researchers found that "those who are most disadvantaged, such as Negroes and recent migrants to the area, will have less access to [informal sources] than others. A special tabulation of the job source data for material handlers and janitors by color shows that the nonwhite workers in those occupations found their present job through formal sources twice as often as whites" (Rees and Schultz, p. 203). This pattern may be changing over time. Parnes (1970) reports finding no race difference in the search behavior of a national sample of 14-to 24-year-old women.

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[2] The basic published source of this study is Rees and Schultz (1970).

This empirical importance of informal labor market channels stands in contrast to most of the recent theoretical work on job search and labor market information, which models the behavior of job seekers as they trade off the costs and benefits of searching out one additional wage offer. As Rees points out,[4] information search involves both an extensive margin and an intensive margin. Search at the extensive margin is characterized by workers obtaining wage offers from additional employers, while search at the intensive margin involves obtaining additional information about job offers already in hand. The former type of search is most likely to be found in markets where the objects of search (e.g., commodities, jobs) are quite homogeneous. Thus, search for additional information in the market for new cars or shares of common stock is most likely to take the form of obtaining price information from additional dealers or brokers. Since jobs are quite heterogeneous with respect to a host of characteristics other than wage, job search is likely to take the form of obtaining additional information about the characteristics of existing job prospects. The same thing is true from the employer side of the market, where the heterogeneity of potential employees dictates search strategies by employers which involve obtaining extensive information from a limited number of job applicants.

A second important finding from the Chicago labor market study was that the jobs which were filled through formal channels generally paid less than jobs filled through informal channels. Ullman (1968) documents this finding and interprets it as the outcome of the alternative "manning" strategies available to employers. He sees employers as facing a three-way tradeoff among the amount of search, wages and the quality of workers hired. Some firms may adopt a "high wage" strategy in which higher than average wages provide a steady stream of applicants through costless informal channels. Firms following this strategy may not deliberately raise wages in order to use the informal channels; instead, high wages may come about for institutional reasons (e.g., unions) and this strategy may merely be a response to the already existing wage structure. Firms with low wages were found to adopt a "high search" strategy which usually involved the use of such formal sources as want ads or private employment agencies.

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[3] Rees (1966), p. 559.

[4] Rees (1966), pp. 560-61.

Differences in training required by jobs and present in prospective employees add a third dimension to search behavior. Firms which provide extensive training to previously untrained employees were hypothesized to adopt a "high training" strategy which involved paying low initial wages and employing formal search methods, often in local schools.

Ullman's conclusions about search methods and wages are summarized in his advice to job seekers: "they should cultivate informal market contacts, rather than relying on (formal) intermediaries for their job search. The highest paid jobs available to a given worker are those he finds informally" (1968, p. 164).

Granovetter (1974) examined job seeking for a sample of 282 male professional and technical workers living in Newton, Massachusetts in the early 1970s who had changed jobs within the past five years. About 57 percent of these men had heard about their current job from a personal contact, and these men had higher incomes than men who used more formal means of finding a job. Those whose personal contacts were occupational rather than social or familial and whose ties to their contacts were weak rather than strong enjoyed the highest incomes. Interestingly, for more than half of all men who heard about their job informally, the contact initiated the interaction in which job information was passed. In about half of these cases, the contact did not know whether the man was looking for a new job. Given the importance of occupational contacts and weak ties, one might suspect that use of personal contacts is correlated with worker quality. Unfortunately, Granovetter's data do not allow definitive tests for this.

Stevens (1978) reviews findings from 34 empirical studies of job seeking and job finding. Although most of these studies are limited to certain geographic locations or to workers in a limited number of occupations, virtually all show that informal methods of job search were used more often than formal methods by

successful job seekers. Stevens points out the bias in making inferences about job-seeking choices from this kind of information on successful job-seeking methods. Merely knowing one successful method of job search or even knowing all the methods actually used tells us little about the costs and benefits of the alternative choices.

THEORETICAL CONSIDERATIONS

A theoretical understanding of informal information and influence networks and their possible wage effects derives from both the supply and the demand sides of the labor market. On the supply side, the question is: Why should individuals with such contacts be able to find the more highly paid jobs among all the possibilities open to them? On the demand side, there are two questions: Why should firms that pay higher wages acquire their employees through informal means? and Why should firms be willing to pay employees acquired informally more than employees acquired formally?

The supply side question can be answered within the framework of the theory of labor market information and job search. For every worker entering the labor market, there are scores of potential employers available to him or her. Fluctuations in product supply and demand conditions, imperfections on either the supply or demand sides of the labor market, and imperfect information regarding job offers produce a distribution of potential wage offers to workers of a given "quality" instead of a uniform wage. ("Wages" include the monetary value of fringe benefits and desirable working conditions.) Knowledge of job openings and wage levels are acquired through search, a process that takes time and perhaps money as well.

The longer and more intensively a worker searches among job offers the higher the maximum offer available to him or her. Workers are presumed to search for wage offers until the expected marginal return (i.e. the marginal wage rate increase from one additional search) equals the marginal cost of search. Factors which lower search costs will increase search time and/or intensity and hence raise the maximum wage offer.

Compared to investigating jobs through want ads, employment agencies, and other means, hearing about a job through a friend, relative, or acquaintance and having access to help in getting jobs are relatively inexpensive methods of finding out about additional job offers. Knowing someone who works at the prospective place of employment would provide much more reliable information about working conditions and advancement possibilities than want ads, job notices posted in an employment agency, or conversations with personnel office employees. Thus, this inside information might speed up the matching process between employee and employer and confer short term wage advantages. The time and money costs of additional search for individuals who have these informal avenues open to them are likely to be much lower than the for others and their search can thus be more extensive and their eventual wages are likely to be higher.

On the demand side, firms that pay higher wages can rely on low-cost informal means of recruiting workers because the employer uses wages and search costs as substitutes in the labor market. The higher the wage rate, the more likely the possibility that high quality workers will accept the offers and the lower the time and effort spent by the firm in search activities.

Informal recruiting would be particularly important in firms and industries characterized by high fixed wages and skill specificity. High fixed wages eliminate the employer's option of paying for higher search costs by lowering wages, so the employer is forced to find ways to minimize those costs. A high degree of skill specificity means that the work consists primarily of tasks that are unique to the firm or industry. Learning those tasks requires large investments by the employer in employee on-the-job training. As a result, hiring becomes a more uncertain process because skills and abilities that prospective employees possess may not closely correspond to tasks they will perform on the job. When informal on-the-job training techniques are employed, a worker's success depends not only on his or her individual potential productivity, but on how well the worker can fit in and get along with fellow workers. Recommendations by experienced workers could provide the employers with information not measured by formal qualifications that would assist in the hiring process. The costs of hiring someone unqualified for the job and, thus, the benefits of relying on informal channels rise as the amount of skill specificity and informal on-the-job training increase.

The dual labor market literature (see Cain) illustrates the ways in which informal recruiting operates to perpetuate race and sex-based wage differences. Since white men hold most of the "good" primary sector jobs and recruiting is informal, young white men will be able to use informal channels to obtain good primary sector jobs. If informal channels available to black men and to women of both races merely lead to jobs held by other blacks and women, then informal information and influence channels will not benefit these groups as much as white men.

This analysis has a number of different implications for the operation of labor markets. First, since skill specificity and fixed relative wages make informal search channels more attractive to employers, the use of these search methods should be more prevalent in occupations where formal educational and experience requirements are less important and where internal labor markets predominate. Second, informal search channels would also be used more frequently in jobs where on-the-job training is done through informal contact with more experienced workers and where the length of the training period is longer. Third, in occupations where informal search is important, the work force in the firm would tend to remain relatively homogeneous. In the case of most high paying jobs where the work force is largely made up of white men, blacks and white women would tend to be excluded from available jobs. Fourth, wages would likely be higher among workers who have informal contacts than among those who have no such contacts.

DATA AND SAMPLES

In this chapter we examine how workers, under 45 years of age heard about and got their first jobs using a sample of individuals who worked at least 250 hours in 1977. The sample of 3,759 included male household heads, their wives, and female household heads, worked at least 250 hours in 1977. Of this total, 1,499 were white men, 667 were black men, 988 were white women and 605 were black women. In the 1978 wave of the Panel Study, household heads were asked to report how they heard about and obtained both their first and their current jobs and, where appropriate, how their wives obtained their current jobs.

Using the sequence of questions as shown in Table 1.1, we constructed ten measures of influence and information--five for the current job and five for the first job, and three dichotomous measures of perceived job quality--two for the current job and one for the first job. These measures are listed in Table 1.2. The construction of most measures should be self-evident. "Current job in demand" is coded 1 for workers who report that many qualified people would be ready and eager to take a job like theirs when it became available.

The first of these five measures of information and influence, "how heard about current/first job," is a measure of job-seeking. Our analyses concentrate on whether a worker learned about a particular job opening through personal contacts since this is a relatively inexpensive way of increasing the job openings applied for in a given period. From the demand side, employers who pay relatively high wages and have low formal requirements may be more likely to rely

Table 1.1 (Page 1 of 2)

SEQUENCE OF QUESTIONS ABOUT JOB ACQUISITION

12

D60. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET AND CHECK ONE:

1. HEAD IS UNDER 45

1. HEAD IS 45-64 YEARS OLD (TURN TO PAGE 14, D83)

5. HEAD IS 65 OR OLDER (TURN TO PAGE 22, G1)

D61. We are interested in how people hear about and get their jobs. How old were you when you first got a job you thought of as a regular or possibly permanent job?

________ AGE

98. HEAD DOES NOT HAVE AND NEVER HAD A REGULAR OR PERMANENT JOB (TURN TO PAGE 13, D72)

D62. What sort of work did you do on that job? ________________________ ☐ OCC

D63. Was that the type of job that gave you a lot of useful skills or training?

1. YES   5. NO

D64. How did you first hear about that job--was it through a friend, a relative, a want ad, an employment agency or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR   2. RELATIVE   3. WANT AD   4. EMPLOYMENT AGENCY   OTHER (SPECIFY): 7. ____________

D65. Do you think there was anyone who may have helped you get the job?

1. YES   5. NO (GO TO D71)

D66. Was that a friend, a relative, or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR   2. RELATIVE   OTHER (SPECIFY): 7. ____________

D67. How did they help? ________________________

D68. Did they work there?

1. YES   5. NO (GO TO D71)

D69. Could they have had some say in your getting the job?

1. YES   5. NO (GO TO D71)

D70. How much say do you think they had? ________________________

(GO TO D71)

D71. Before you got the job, did you know anyone (else) who worked there?

1. YES   5. NO

Table 1.1 (Page 2 of 2)

13

D72. Are you still working for that same employer or are you now working for a different one?

1. SAME (TURN TO PAGE 22, G1)   5. DIFFERENT

D73. How long have you been working for your present employer?

__________ MONTHS OR __________ YEARS

D74. How did you first hear about a job with your present employer--was it through a friend, a relative, a want ad, an employment agency, or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR   2. RELATIVE   3. WANT AD   4. EMPLOYMENT AGENCY

7. OTHER (SPECIFY): ____________________

D75. Is this the type of job that gives you useful training or skills?

1. YES   5. NO

D76. Do you think there was anyone who may have helped you get the job?

1. YES   5. NO (GO TO D82)

D77. Was that a friend, relative, or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR   2. RELATIVE   7. OTHER (SPECIFY): ________________

D78. How did they help? ______________________________

____

D79. Did they work for your present employer?

1. YES   5. NO (GO TO D82)

D80. Could they have had some say in your getting the job?

1. YES   5. NO (GO TO D82)

D81. How much say do you think they had? ____________________

____

D82. Before you got your first job with your present employer, did you know anyone (else) who worked there?

1. YES   5. NO

(TURN TO PAGE 22, G1)

Table 1.2 (page 1 of 2)

MEAN VALUES ON INFORMATION, INFLUENCE AND PERCEIVED JOB QUALITY
(For male household heads, female household
heads, and wives under 45 years of age
who worked more than 250 hours in 1977.)

Measures	White Men	White Women	Black Men	Black Women
Current Job Measures				
How heard about current job?				
From friends or relatives	.520	.471	.585	.430
Want ad	.094	.142	.069	.110
Employment agency	.058	.058	.070	.152
Knew someone on current job	.566	.470	.675	.501
Received help on current job	.403	.314	.430	.337
Direct influence (stated and inferred)	.137	.090	.107	.042
They recommended me	.133	.112	.150	.075
They told employer about me	.056	.038	.079	.047
They told me about the job	.050	.043	.074	.133
Knew someone and received help	.333	.244	.395	.246
Knew someone and received help from someone with much say	.184	.132	.166	.076
Current job provides skills	.835		.784	
Current job in demand	.418		.566	

on word-of-mouth to attract job applicants rather than more formal methods. The second measure is whether a worker "knew someone on current/first job" before beginning work. Personal contacts who are already working for a potential employer may provide job-seekers with useful information about work conditions, benefits, and wage growth, thus increasing the amount of information gained about jobs for a given period of job search. Similarly, current employees are a cheap source of information to employers about potential workers. Employee evaluations of job-seekers may be particularly useful in the jobs which do not require extensive formal qualifications but which do include a great deal of on-the-job training by co-workers. The third variable, "received help on current/first

Table 1.2 (page 2 of 2)

Measures	White Men	White Women	Black Men	Black Women
First Job Measures				
How heard about first job?				
From friends or relatives	.555		.671	
Want ad	.075		.030	
Employment agency	.050		.051	
Know someone on first job	.572		.701	
Received help on first job	.434		.549	
Direct influence (stated and inferred)	.177		.128	
They recommended me	.105		.204	
They told employer about me	.066		.112	
They told me about job	.043		.077	
Knew someone and received help on first job	.372		.473	
Knew someone and received help from someone with much say on first job	.211		.198	
First job provides skills	.673		.622	
N	1499	988	667	605

job," measures whether a worker received any help at all from personal contacts in getting that job. The kind of help received is broken down into four categories based on a priority order of responses to the question "How did they help?" A direct influence was coded if a respondent stated that the person who helped him or her "gave me the job," "got me the job," or was "a friend of the foreman." The other categories are clear. Unlike the first two measures, this help measure taps influence as well as information. Presumably people who receive help from friends are likely to generate more or better job offers per given period than otherwise similar people who receive no such help. The last two measures are interactions: "knew someone and received help on current/first job" and "knew someone and received help from someone with much say on current/first job." This last variable measures whether a worker received help in getting a job from a person with much say who also worked for the firm.[5] These interactions tap both influence and information. By receiving help from someone already working in a firm, a jobseeker increases the number of job offers and information about the quality of a potential job. Firms which rely on employee information to fill jobs should be those where formal credentials are less important and/or where informal on-the-job training by co-workers is extensive. Furthermore, given the importance of personal contacts in employee selection, such firms may be more likely than others to have a homogeneous work force. The measures of perceived job quality are quite general. Workers reported whether their jobs gave them a lot of useful skills or training ("current/first job provides skills") and whether their current job would attract many applicants ("current job in demand").[6]

Husbands, not wives, reported on how their wives obtained their current jobs. Husbands were not asked about their wives' first jobs because we felt this information would be less reliable. For this reason, we only examine sex differences in how workers obtained their current jobs. There is an additional problem. It might be expected that husbands would be more likely to misreport how their wives obtained their jobs than how they obtained their own jobs. If this were the case, observed sex differences in information and influence channels may be due to sex differences in measurement error.

The five measures of information and influence in getting current jobs are not highly correlated (with the obvious exception of interaction terms). For instance, correlations between "hearing from a friend or relative," "knowing someone," and "received help from someone" range from .289 to .462 (See Table 1.3). The correlations of the two interaction measures with "hearing from a friend or relative" are also quite low, ranging from .219 to .400. Correlations between the influence and information measures for first jobs exhibit a similar pattern (Table 1.4).

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[5] The Panel Study only asked workers who had received help from someone in the firm if that person had much say. Thus, we have no measure of whether a worker received help from someone with much say who did not work for the employer.

[6] The question for this latter variable was "When a job like yours becomes available, would there be many qualified people ready and eager to get it, very few or what?"

Table 1.3

CORRELATION MATRIX FOR INFORMATION AND INFLUENCE VARIABLES ON CURRENT JOB, BY RACE AND SEX
(For male household heads, female household heads, and wives under 45 years of age who worked more than 250 hours in 1977.)

	Knew Someone on Current Job	Received Help on Current Job	Knew Someone and Received Help on Current Job	Knew Someone and Received Help from Someone with Much Say on Current Job
Heard about current job from a friend or relative	.416=White men	.379	.389	.268
	.462=White women	.405	.400	.299
	.457=Black men	.289	.341	.219
	.300=Black women	.407	.341	.229
Knew someone on current job		.431	.618	.417
		.414	.603	.415
		.451	.561	.309
		.329	.571	.286
Received help from someone on current job			.860	.580
			.839	.577
			.930	.513
			.802	.402
Knew someone and received help from someone on current job				.675
				.688
				.551
				.501

Table 1.4

CORRELATION MATRIX FOR INFORMATION AND INFLUENCE VARIABLES ON FIRST JOB BY RACE

(For employed male household heads under 45 years of age who worked more than 250 hours in 1977.)

	Knew Someone on First Job	Received Help on First Job	Knew Someone and Received Help on First Job	Knew Someone and Received Help from Someone with Much Say on First Job
Heard about first job from a friend or relative	.514=White men .390=Black men	.461 .324	.474 .303	.343 .301
Knew someone		.502 .390	.665 .619	.448 .325
Received help			.878 .860	.592 .451
Knew someone and received help				.674 .525

HOW DID WORKERS HEAR ABOUT AND GET THEIR JOBS?

Friends, relatives, and personal contacts were a major source of information and help to workers seeking jobs (Table 1.2). About half of all workers heard about their current job through a friend or relative and about half knew someone who worked for their current employer before they began work. Reports of influence, while less common, were still numerous. About 40 percent of the men and one-third of the women reported that someone helped them get their current job, about one-third of the men and one-quarter of the women reported that they both knew someone and received help, and more than one-sixth of the men and one-tenth of the women reported that they had been helped by someone with much say who also worked for their current employer.

Turning to first jobs we see that men were somewhat more likely to have used informal information and influence channels in getting their first jobs than in getting their current one. This suggests that informal networks may be most important early in a worker's career. Nonetheless, the patterns across the five measures are quite similar for both first and current jobs. The majority of men heard about their first job from a friend and/or knew someone who worked there before taking the job. Somewhat fewer men (43 percent of whites and 55 percent of blacks) received help in getting the first job; still fewer of both knew someone and received help, and only one-fifth received help from someone with much say who worked for their first employer.

WHICH WORKERS USE INFORMAL CHANNELS?

In contrast to previous work (Rees and Schulz), we found that black men were more likely than white men to have heard about a job from a friend. In addition, they were more likely to have known someone who worked there, to be helped by someone in getting the job, and to both know someone and be helped by someone. Black and white men were equally likely to have been helped by someone with much say. If we look at the kind of help received, we see that white men were more likely than black men to report having received direct help. These patterns held for both the first and current job, although race differences were more pronounced for the first job.

According to these data, women were considerably less likely than men to have used informal information and influence channels in obtaining their current jobs. Recall, however, that husbands reported on their wives' use of such channels. We would expect such a sex difference if husbands systematically underestimated the extent to which their wives had access to friendship networks to learn about and get jobs. To check this, we compared women's use of informal information and influence channels by marital status (Table 1.5). According to reports by female household heads and for wives, women were equally likely to have used such channels. Black female heads, on the other hand, consistently reported more use of such channels than did black wives. This information could be accurate rather than the result of husbands' misreporting, since the pressure to get high wage jobs is stronger for female heads. However, black female heads reported considerably less use of such channels than did black men and slightly less than did white men.

Regardless of race/sex group, workers with college and advanced degrees and professional workers were considerably less likely than the average worker to report having used informal information and influence channels in obtaining their current jobs (Tables 1.6 and 1.7). While over half (52 percent) of all white men reported having heard about their current job from a friend, only 32 percent of white male professionals, only 39 percent of white men with BAs, and only 23 percent of white men with advanced degrees reported having heard about their current job from a friend. Forty percent of all white men reported that someone helped them get their present job, but only one-third of white men with professional jobs, college degrees, or advanced degrees reported such help. This pattern also held true for the first jobs of white and black male professionals (see Table 1.8). Perhaps informal information and influence networks tend to substitute for the more formal credentials used by well-educated and professional workers.

White men in blue collar occupations were more likely than other white men to have heard about their jobs from a friend, to have known someone on the job, to have been helped by someone, to have both known and been helped by someone and to have been helped by someone with much say--both on their current and first jobs. This was generally true but less dramatic for the first jobs of black blue collar workers and was not true at all for their current jobs.

Table 1.9 presents the results when the five information and influence measures are regressed on schooling, job tenure, and age at the time of hire for each race-sex group. For white men, education and age were negatively associated with all five information and influence measures on both first and current jobs. That is, the younger and less educated a white man was when he obtained a job, the more likely it was that he had heard about that job from friends or relatives, that he knew someone who worked there, that someone helped him get the job, and that he both knew someone and someone helped him get the job. Part of

Table 1.5

DIFFERENCES IN INFORMATION AND INFLUENCE
IN GETTING CURRENT JOB BY MARITAL STATUS
(For female household heads and wives under 45
years of age who worked more than 250 hours in 1977.)

Marital Status	Heard About Current Job from a Friend or Relative		Knew Someone on Current Job		Received Help on Current Job		Knew Someone and Received Help on Current Job		Knew Someone and Received Help from Someone with Much Say on Current Job	
	White Women	Black Women	White Women	Black Women	White Women	Black Women	White Women	Black Women	White Women	Black Women
Female head	.487	.464	.463	.557	.336	.385	.255	.303	.128	.085
Wife	.464	.399	.474	.451	.304	.294	.238	.196	.134	.068
Eta^2 (adj)	.000	.004	.000	.011	.001	.009	.000	.015	.000	.000

Table 1.6 (page 1 of 2)

INFORMATION AND INFLUENCE ON FIRST JOB WITH CURRENT EMPLOYER BY OCCUPATION
(For male household heads, female household heads, and wives, under 45 years of age who worked more than 250 hours in 1977.)

Current Occupation	Heard About Current Job from a Friend or Relative				Knew Someone on Current Job				Received Help on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Major Census Category												
Professional	31.9%	36.5%	49.1%	41.4%	43.4%	40.2%	53.7%	44.8%	32.5%	26.5%	30.6%	33.0%
Manager	48.1[a]	41.0[a]	41.7[a]	18.2[a]	52.8[a]	37.0[a]	79.5[a]	21.6[a]	43.9[a]	33.0[a]	35.8[a]	18.2[a]
Clerical and sales	51.6	47.1	61.6	34.5	53.7	45.1	69.8	37.8	40.1	35.8	40.2	33.2
Foremen & craftsmen	58.1	b	61.7	70.4[a]	63.6	b	69.4	80.0[a]	41.6	b	44.7	64.2[a]
Military, police, fire	46.1	b	37.1	b	48.9	b	34.5	b	24.0	b	47.4	b
Transport operatives	69.8	35.9[a]	69.6	b	64.3	53.6[a]	67.7	b	53.8	24.5[a]	34.7	b
Other operatives	61.2	63.6	60.3	52.2	64.3	71.6	74.6	72.8	41.6	23.1	42.4	21.5
Laborers, household workers	74.6	b	52.9	78.1[a]	67.0	b	61.2	27.3[a]	51.1	b	38.6	76.6[a]
Other service	42.5	53.6	59.9	45.8	60.4	47.7	66.1	55.8	34.6	32.3	60.7	38.1
Other	54.5[a]	45.5	b	35.5[a]	45.5[a]	48.3	b	87.1[a]	46.9[a]	27.8	b	58.1[a]
Mean	52.0	47.1	58.5	43.0	56.6	47.0	67.5	50.1	40.3	31.4	43.0	33.7
Eta squared (adj)	.060	.032	.018	.057	.028	.037	.026	.107	.019	.012	.924	.969
Whether or Not Blue Collar												
White collar	41.6	43.2	56.8	34.9	48.7	42.9	68.1	38.3	37.7	32.6	37.9	32.6
Blue collar	62.0	61.6	60.0	55.7	64.2	68.2	68.2	66.0	43.6	25.2	42.0	31.7
Other	44.2	53.0	56.2	45.7	50.0	47.5	63.7	57.2	31.6	31.7	57.0	38.5
Eta squared (adj)	.041	.018	.001	.031	.024	.020	.001	.060	.006	.003	.965	.993

Table 1.6 (page 2 of 2)

Current Occupation	Knew and Received Help from Someone on Current Job				Knew and Received Help from Someone with Much Say on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Major Census Category								
Professional	22.3%	19.4%	27.3%	22.7%	12.9%	9.2%	23.6%	17.0%
Manager	35.6	25.9	33.8	18.2	27.0	12.9	6.6	0.0[a]
Clerical and sales	33.6	28.4	40.0	25.7	17.9	15.9	21.5	6.0
Craftsmen and foremen	35.7	b	39.6	62.4[a]	17.0	b	13.5	0.8[a]
Military, police fire	16.6	b	29.3	b	6.8	b	5.2	b
Transport operatives	44.9	24.5[a]	33.7	b	28.1	22.9[a]	10.9	b
Other operatives	38.8	21.1	40.1	18.1	20.1	9.0	15.3	11.2
Laborers, household workers	41.3	b	29.4	27.3	24.8	b	12.3	7.8[a]
Other service	34.2	20.6	58.8	24.5	10.6	12.9	24.3	4.9
Other	38.3[a]	21.4	b	58.1[a]	8.7[a]	17.9	b	19.4[a]
Mean	33.3	24.4	39.5	24.6	18.4	13.2	16.6	7.6[a]
Eta^2 (adj)	.025	.012	.031	.035	.023	.010	.019	.022
Whether or not Blue-Collar								
White-collar	29.0	25.3	36.9	24.8	18.5	13.6	20.0	7.6
Blue-collar	37.9	23.6	36.8	23.5	19.8	11.1	13.1	9.3
Other	26.1	20.8	55.2	25.4	7.8	13.4	23.3	5.4
Eta squared (adj)	.010	.001	.017	.000	.006	.001	.012	.002

[a] Based on 10-19 cases.
[b] Based on less than 10 cases.

Table 1.7 (page 1 of 2)

INFORMATION AND INFLUENCE ON FIRST JOB WITH CURRENT EMPLOYER BY EDUCATION
(For male household heads, female household heads, and wives under 45 years
of age who worked more than 250 hours in 1977)

Education	Heard About Current Job from a Friend or Relative				Knew Someone on Current Job				Received Help on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
8 or fewer grades	69.3%	53.4%	60.4%	26.6%	77.5%%	66.2%	76.1%	68.5	48.7%	27.4%	53.1%	56.5%
9-11	59.5	61.0	64.9	41.1	60.7	57.7	74.2	68.5	40.0	29.4	45.3	28.3
12	64.2	51.0	55.6	45.7	66.2	53.0	73.7	46.3	46.3	31.8	50.0	29.9
12+ non acad.	51.8	42.4	49.2	44.2	56.5	42.6	56.8	33.3	40.9	34.7	47.0	40.8
13-15	53.4	45.2	68.8	49.0	58.1	41.0	60.8	49.6	41.7	32.1	28.1	41.6
B.A.	38.5	37.1	38.3	24.7	40.6	43.4	54.6	41.0	33.9	31.4	31.6	27.2
Adv. degree	22.8	50.2	b	b	43.6	31.9	b	b	27.1	23.1	b	b
Mean	52.0	47.1	58.5	43.0	56.6	47.0	67.5	50.1	40.3	31.4	43.0	33.7
Eta^2 (adj.)	.056	.018	.034	.016	.037	.022	.034	.059	.012	.004	.037	.021

Table 1.7 (page 2 of 2)

Education	Knew and Received Help on Current Job				Knew and Received Help from Someone with Much Say on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
8 or fewer grades	45.0%	27.4%	50.9%	53.2%	11.1%	19.3%	17.0%	10.5%
9-11	35.1	27.3	42.7	25.2	19.9	19.9	9.4	12.5
12	41.7	26.8	43.5	19.7	23.3	12.9	22.7	3.2
12 + non acad.	32.1	25.7	44.7	17.7	17.0	13.5	9.9	8.5
13-15	35.6	21.4	26.3	38.6	22.5	10.1	21.3	5.9
B.A.	23.5	23.3	29.1	21.9	12.1	13.3	11.2	12.7
Adv. degree	16.4	14.5	b	b	12.3	10.4	b	b
Mean	33.3	24.4	39.5	24.6	18.4	13.2	16.6	7.6
Eta^2(adj.)	.026	.006	.030	.038	.013	.007	.025	.020

[a] Based on 10-19 cases.
[b] Based on less than 10 cases.

Table 1.8 (page 1 of 2)

INFLUENCE AND INFORMATION ON FIRST JOB BY FIRST OCCUPATION AND BY EDUCATION

(For male household heads under 45
years of age who worked more than 250 hours in 1977)

First Occupation	Heard About First Job from a Friend or Relative		Knew Someone on First Job		Received Help on First Job		Knew Someone and Received Help on First Job		Knew and Received Help from Someone with Much Say on First Job	
	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men
Major Census Category										
Professional	20.0%	56.3%[a]	35.7%	54.7%[a]	27.6%	28.1%[a]	21.0%	24.2%[a]	8.3%	20.3%[a]
Manager	46.1	b	58.2	b	43.7	b	39.6	b	24.9	b
Clerical & sales	51.4	72.6	48.7	65.6	35.7	37.9	29.8	36.0	17.1	23.3
Craftsmen & foremen	66.8	72.3	62.3	59.2	48.3	46.6	39.4	37.7	25.8	31.4
Operatives	65.6	71.3	66.9	69.9	47.6	47.6	45.4	44.0	26.3	16.4
Unskilled labor and service	68.4	72.2	63.0	73.4	55.3	63.6	47.0	52.8	28.2	21.5
Other	51.4	50.6	58.7	71.9	32.8	55.9	27.3	49.5	11.6	15.3
Mean	55.5	67.1	57.2	70.1	43.4	54.9	37.2	47.3	21.1	19.8
Eta squared (adj)	.117	.063	.046	.011	.041	.039	.041	.019	.036	.012
Whether or Not Blue-Collar										
White collar	33.3	61.0	42.4	61.7	31.8	38.3	25.7	35.4	12.9	20.0
Blue-collar	67.1	72.0	64.1	71.5	51.9	58.2	44.5	49.4	27.0	20.8
Other	51.6	50.6	59.0	71.5	33.3	55.9	27.7	49.5	11.8	15.3
Eta squared (adj)	.091	.028	.038	.005	.035	.020	.034	.010	.030	.002

Table 1.8 (page 2 of 2)

Education	Heard About First Job from a Friend or Relative		Knew Someone on First Job		Received Help on First Job		Knew Someone and Received Help on First Job		Knew and Received Help from Someone with Much Say on First Job	
	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men
8 or fewer grades	68.3%	64.5%	73.9%	63.8%	55.1%	42.8%	51.2%	34.6%	18.7%	17.6%
9-11	68.8	72.1	63.6	68.6	48.3	59.0	42.8	45.9	30.8	15.6
12	68.9	68.9	71.4	76.1	51.5	60.2	46.9	56.9	26.9	26.3
12 + non-acad	59.6	63.6	57.5	73.5	47.7	48.6	41.7	46.2	24.6	13.6
13-15	56.4	68.9	53.3	64.7	40.6	53.7	31.4	40.2	16.8	17.3
B.A.	35.6	49.5	44.0	66.3	31.5	52.6	25.7	49.0	12.8	29.1
Adv. degree	19.4	b	37.7	b	31.1	b	23.6	b	12.9	b
Mean	55.5	67.1	57.2	70.1	43.4	54.9	37.2	47.3	21.2	19.8
Eta squared (adj)	.091	.021	.047	.023	.025	.020	.032	.027	.026	.029

[a] Based on 10-19 cases.
[a] Based on less than 10 cases.

the negative associations with education and age may be an occupation effect. That is, well-educated and older workers may be more likely to seek work in occupations which emphasize formal credentials and past experience or training. Results when occupation is controlled suggest that this may be true for schooling but not for age at the time of hire. When occupation is controlled (Table 1.10) the magnitude of the negative association between schooling and the information and influence measures for first and current jobs dropped sharply for white men. But controls for occupation had no effect on the negative association for the current job between the age and the influence and information measures and only slightly reduced the negative association for the first job.

For other race/sex groups, amount of schooling and the age at the time of hire for the first job were not consistently associated with the measures of informal information and influence. For black men, schooling was negatively and significantly associated with knowing someone, having received help, and both knowing someone and having received help on current jobs, but these associations dropped sharply and became insignificant once occupation was controlled.

WAGE EFFECTS OF INFORMATION AND INFLUENCE

The wage effects of informal information and influence networks were investigated through a series of multiple regressions. The set of three information and influence variables was added to a conventional wage equation that consisted of the natural logarithm of 1977 hourly wages as the dependent variable and the following independent variables: years of formal education, years of pre-employer work experience (entered in quadratic form), years with present employer,[7] the natural logarithm of city size, and a dichotomous measure of whether the respondent lived in the South. To investigate the temporal pattern of wage effects, an additional set of regressions was run that added the interaction between each information and influence variable and years with present employer. All regressions were run with and without controls for a ten-category occupational classification. Different results in the presence of occupational controls would indicate that the wage effect is partly a function of differences in the occupational distribution. The regression results are presented in Table 1.11.

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[7] In previous work (Corcoran and Duncan, 1978), a quadratic form was found to be appropriate for pre-employer experience but not for experience with the current employer.

Table 1.9 (page 1 of 3)

REGRESSIONS ON ACCESS TO INFORMATION AND INFLUENCE ON FIRST AND CURRENT JOB
(For male household heads, female household heads and wives under 45 years of age who worked more than 250 hours in 1977)

Independent Variables	Whether Heard about Current Job from A Friend or Relative				Whether Knew Someone on Current Job				Whether Received Help on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Education	-.039**	-.015+	-.010	-.013	-.032**	-.026**	-.023**	-.035**	-.014**	-.005	-.029**	-.004
	(.005)	(.007)	(.009)	(.011)	(.005)	(.007)	(.009)	(.011)	(.005)	(.006)	(.009)	(.011)
Tenure	-.002	.002	-.000	-.011*	-.001	-.004	.003	.006	-.005*	-.006+	.001	-.004
	(.003)	(.004)	(.004)	(.005)	(.002)	(.004)	(.004)	(.005)	(.003)	(.004)	(.004)	(.005)
Age took current job	-.007**	.005*	.001	-.004	-.001	.002	-.003	-.000	-.006**	-.003	-.002	.004
	(.002)	(.003)	(.004)	(.003)	(.002)	(.003)	(.004)	(.003)	(.002)	(.002)	(.004)	(.003)
Constant	1.217	.539	.687	.741	1.017	.762	1.007	.890	.762	.482	.809	.303
R^2(adj.)	.048	.007	.000	.004	.027	.014	.010	.016	.010	.001	.012	.000

Table 1.9 (page 2 of 5)

Independent Variables	Whether Knew Someone and Received Help on Current Job				Whether Knew Someone and Received Help from Someone with Much Say on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Education	-.022** (.005)	-.013* (.006)	-.028** (.009)	-.007 (.009)	-.007+ (.004)	-.008+ (.005)	.009 (.007)	-.003 (.006)
Tenure	-.004+ (.002)	-.004 (.003)	.004 (.004)	-.004 (.004)	-.003 (.002)	-.009** (.002)	-.002 (.003)	-.001 (.003)
Age took current job	-.007** (.002)	-.002 (.002)	-.005 (.004)	.002 (.003)	-.004* (.002)	.000 (.002)	-.003 (.003)	.002 (.002)
Constant	.810	.472	.814	.285	.393	.259	.157	.063
R^2(adj.)	.021	.003	.017	.000	.004	.014	.001	.000

Table 1.9 (page 3 of 3)

Independent Variables	Whether Heard about First Job from A Friend or Relative		Whether Knew Someone on First Job		Whether Received Help on First Job		Whether Knew Someone and Received Help on First Job		Whether Knew Someone and Received Help from Someone with Much Say on First Job	
	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men
Education	-.035** (.005)	.002 (.009)	-.022** (.005)	-.017* (.009)	-.012* (.006)	.004 (.010)	-.017** (.005)	-.006 (.020)	-.008+ (.005)	.006 (.008)
Age	.004* (.002)	.006* (.003)	.001 (.002)	-.005+ (.003)	-.002 (.002)	.000 (.003)	-.002 (.002)	-.005+ (.003)	-.002 (.002)	-.006* (.003)
Age took first job	-.028** (.004)	-.032** (.006)	-.024** (.004)	.011+ (.006)	-.024** (.004)	-.013* (.006)	-.020** (.004)	.007 (.006)	-.019** (.004)	-.010* (.005)
R^2(adj.)	.110	.045	.053	.007	.039	.002	.041	.001	.034	.014
Constant	1.693	1.074	1.288	.849	1.101	.734	1.045	.561	.725	.503

+Significant at .10 level.
*Significant at .05 level
**Significant at .01 level

Table 1.10 (page 1 of 3)

REGRESSION ON ACCESS TO INFORMATION AND INFLUENCE ON FIRST AND CURRENT JOB [OCCUPATION CONTROLLED][a]
(For male household heads, female household heads and wives under 45 years of age who worked more than 250 hours in 1977)

Independent Variables	Whether Heard about Current Job from Friend or Relative				Whether Knew Someone on Current Job				Whether Received Help on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Education	-.021** (.006)	-.008 (.009)	.006** (.001)	.011 (.014)	-.021** (.006)	-.016+ (.009)	-.031** (.011)	.002 (.014)	-.010 (.006)	-.004 (.008)	-.035** (.011)	-.011 (.014)
Tenure	.000 (.003)	.003 (.004)	.000 (.004)	.010* (.005)	.001 (.003)	-.004 (.004)	.003 (.004)	.014** (.005)	-.004 (.003)	-.006 (.004)	.003 (.003)	-.010* (.003)
Age when took job	-.006** (.002)	.005* (.003)	.001 (.004)	-.004 (.003)	-.000 (.002)	.002 (.003)	-.003 (.004)	.003 (.003)	-.007** (.002)	-.002 (.002)	-.001 (.004)	.001 (.003)

Table 1.10 (page 2 of 3)

Independent Variables	Whether Knew Someone and Received Help on Current Job				Whether Knew Someone and Received Help from Someone with Much Say on Current Job			
	White Men	White Women	Black Men	Black Women	White Men	White Women	Black Men	Black Women
Education	-.016** (.006)	-.015+ (.008)	-.039** (.010)	-.021+ (.013)	-.006 (.005)	-.008 (.006)	.006 (.009)	-.006 (.008)
Tenure	-.003 (.003)	-.004 (.003)	.006 (.004)	-.005 (.005)	-.003 (.002)	-.009** (.003)	-.001 (.003)	-.002 (.003)
Age when took job	-.007** (.002)	-.001 (.002)	-.003 (.004)	.001 (.003)	-.005** (.002)	.001 (.002)	-.003 (.003)	.003 (.002)

Table 1.10 (page 3 of 3)

Independent Variables	Whether Heard about First Job from A Friend or Relative		Whether Knew Someone on First Job		Whether Received Help on First Job		Whether Knew Someone and Received Help on First Job		Whether Knew Someone and Received Help from Someone with Much Say on First Job	
	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men	White Men	Black Men
Education	-.017** (.006)	.008 (.009)	-.012* (.006)	-.009 (.010)	-.006 (.006)	.014 (.010)	-.011+ (.006)	.005 (.020)	-.003 (.005)	.009 (.008)
Age	-.003+ (.002)	.010** (.003)	.001 (.002)	-.005+ (.003)	-.001 (.002)	.000 (.003)	-.001 (.002)	-.006+ (.003)	-.001 (.002)	-.006* (.003)
Age took first job	-.020** (.004)	-.028** (.006)	-.020** (.005)	.015* (.006)	-.020** (.003)	-.007 (.007)	-.017** (.005)	.012+ (.007)	-.016** (.004)	-.008 (.005)

+=Significant at .10 level
*=Significant at .05 level.
**=Significant at .01 level.
[a] All regressions include dummies for major occupational categories

Table 1.11 (page 1 of 2)

ADDITIVE WAGE EFFECTS OF INFORMATION AND INFLUENCE VARIABLES ON CURRENT JOB[a]
(All household heads and wives age 18-64)

	White Men		White Women		Black Men		Black Women	
	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation
Regression #1								
Heard from friend or relative	-.020 (.023)	-.016 (.023)	-.018 (.030)	.011 (.029)	.081* (.034)	.084** (.032)	-.085* (.035)	-.053 (.034)
Knew Someone	.036 (.023)	.032 (.023)	-.013 (.030)	-.026 (.029)	-.005 (.038)	-.044 (.037)	.033 (.035)	.030 (.035)
Received Help	-.017 (.023)	-.020 (.023)	-.013 (.031)	-.009 (.031)	-.000 (.034)	.031 (.032)	.093* (.037)	.103** (.037)
R^2	.287	.314	.288	.345	.275	.362	.269	.342
Regression #2								
Heard from friend or relative	-.037 (.033)	-.025 (.032)	-.007 (.043)	.018 (.040)	-.087+ (.052)	-.056 (.050)	-.124* (.051)	-.093+ (.050)
Heard from friend or relative times years with present employer	.003 (.004)	.002 (.004)	-.003 (.007)	-.002 (.007)	.030** (.007)	.024** (.007)	.005 (.008)	.006 (.008)

Table 1.11 (page 2 of 2)

Regression #2 (Con't.)	White Men		White Women		Black Men		Black Women	
	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation	Without Controlling for Occupation	Controlling for Occupation
Knew someone	.093** (.034)	.089** (.034)	.044 (.042)	.035 (.040)	.121* (.055)	.085 (.052)	.214** (.049)	.173** (.048)
Knew someone times years with present employer	-.010* (.004)	-.010* (.004)	-.014+ (.007)	-.016* (.007)	-.020** (.008)	-.021** (.007)	-.040** (.008)	-.032** (.008)
Received help	.012 (.033)	.013 (.033)	-.000 (.044)	.011 (.042)	.068 (.048)	.102* (.046)	.092+ (.054)	.117* (.053)
Received help times years with present employer	-.005 (.004)	-.006 (.004)	-.003 (.008)	-.005 (.008)	-.015* (.007)	-.015* (.007)	-.000 (.009)	-.003 (.009)
R^2	.291	.318	.293	.351	.296	.379	.298	.359
Number of observations	1499		988		667		604	

[a] All regressions include years of education, pre-employer work experience, pre-employer work experience squared, years with present employer, whether South and ln city size.

+Statistically significant at .10 level.

*Statistically significant at .05 level.

**Statistically significant at .01 level.

It is apparent that knowing someone on the prospective job confers some general short-term wage advantages but that influence has more erratic effects, mostly for blacks. Black men are the only group for whom merely having heard about the job from a friend was associated with higher wages. As shown in the top half of Table 1.11, black men with this informal information earned about 8 percent higher wages than otherwise similar blacks--a statistically significant difference. The positive coefficient on the interaction term with years with present employer indicates that these wage benefits are not temporary --if anything, they increase with tenure. For each of the other race/sex subgroups, wage effects of hearing about jobs from friends or relatives are generally negative and almost always statistically insignificant.

The most consistent wage effects are associated with having known someone on the job before taking it. As argued earlier, such knowledge is likely to provide much better information about the job itself than could be obtained through a formal application and interview. It was expected that this informal information might be more efficient in matching people with jobs, and that individuals obtaining these jobs might receive short-term wage benefits because of it. That indeed appears to be the case. On average, the wages of those who knew someone on the job before they took the job did not differ much from those without that knowledge. The consistently significant results for the interaction specification indicate, however, that there are indeed short term wage advantages to workers who hold those jobs. The coefficient on the "knew someone" variable in the interactive specification can be interpreted as the wage payoff to knowing someone for those who have just taken jobs with their current employers. These payoffs were positive for all groups, were statistically significant at conventional levels for all but white women, and dropped only slightly when the occupational control variables were added. White men who began on jobs where someone they knew also worked earned about 9 percent more at first; for black men and for women, those wage differences are estimated at about 12 and 21 percent, respectively. The significant coefficients on all of the interaction terms indicate that this wage advantage is temporary, however, declining to zero within nine years for white men, within about five or six years for black men and women, and within three years for white women.[8]

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[8] It is possible that some of this decline is due to the self-selection process associated with job tenure. If unmeasured differences between short and long term employees led the latter to be less productive than the former group, then a similar wage pattern would emerge. That seems unlikely, however, since employers are most likely to keep training most productive workers.

Receiving help in obtaining jobs with current employers was associated with higher average wages among black women and initially higher wages among black men. The wage advantage for black women amounted to about 10 percent--a difference which did not decline over time. For black men, the wage advantage was at the margin of statistical significance at the time of hire and declined by about one and one-half percentage points per year. On average, however, there was no significant wage difference between black men who obtained their jobs with some help and those that didn't. It is puzzling that black men are the only group for whom influence channels affected wages. Since young workers were most likely to have heard about jobs held by older workers in their race and sex group and white men hold the highest paying jobs, it was expected that the wage effects would be most apparent for white men.

The robustness of these results were investigated in two additional ways. A four-category pattern variable was formed from the "knew someone" and "received help" measures as a check for a possible interaction between these two variables. The results indicated that an additive model was appropriate. The wage effects for workers who reported that they received help from someone with much say were compared to the wages of those who had help from someone without much say. Since the "much say" questions were asked only of those people who knew someone who worked on the job before they went to work there, a general comparison was not possible. "Much say" had significant wage effects for both white men and white women, but in the negative direction for white men. Results for black men indicated a wage advantage for those who had contacts with persons with much say which was on the margin of statistical significance ($t = 1.65$); results for black women were totally insignificant.

SUMMARY

Previous empirical work on information and influence in job acquisition have focused on who uses informal techniques to find jobs and the effects that information and influence have on wages received. The results of this previous work indicate that white workers tend to use these channels more than black workers, and workers in jobs where formal credentials are more important tend to use them less. The results presented here are consistent with the latter finding in that, regardless of race/sex group, workers with college and advanced degrees and professional workers are considerably less likely than others to report having used informal information and influence channels in obtaining their current jobs. This pattern was also true for the first jobs of white men and for black male professionals. The results of this analysis, however, do not support the view that black workers are much less likely to use informal means than whites. Instead, black men were slightly more likely to hear about a job from a friend, to know someone who worked at the prospective place of employment, and to receive help in acquiring jobs. Women were considerably less likely than men to have used informal information and influence channels in obtaining their current jobs, and for women differences by race were small.

Findings of previous empirical and theoretical work on the effects of information and influence on wages received suggest that jobs filled through formal channels generally paid less than jobs filled through informal channels. The rationale was that the employer uses wages, search costs, and training costs as substitutes in the labor market. The higher the wage rate, the more quickly high quality workers will accept the wage offers and the lower the amount of time required for search and training activities. Of the results presented here, the one that is most consistent across all race/sex groups is that knowing someone on the job before taking it yields short run wage advantages. This implies that this type of informal information may be more efficient at matching people with jobs since it would provide much better information about the job itself than would a formal application and interview. We also found that black men received long term positive wage benefits from hearing about a job from a friend and short term wage advantages from receiving help in obtaining jobs. Black women enjoyed long term wage increases from receiving help. Thus, the results regarding wage effects do not provide evidence of widespread, permanent wage gains made through information or influence channels.

Looking at both sets of results we find that the use of informal channels and the effect of information and influence on wages do not provide a convincing story to explain early wage differences between blacks and whites. Blacks use informal methods at least as often as whites. Furthermore, whites do not experience higher wage advantages through these channels than blacks. This suggests either that the indicators of influence and information used here are not fine enough to capture black/white differentials or that such channels are not important in understanding the gap between black and white earnings.

It was our finding, published in Volume VI of this series, that differences in human capital and quality of attachment to the labor force could not explain much of the sex and race differences in earnings. That finding led to the present investigation of whether help and influence in getting into the right occupations could explain differences in earnings. In general, the answer is not to any substantial extent. What then does account for the concentrations of females and blacks in jobs with lower pay and less chance for advancement? And how could one test any alternative explanations? Retrospective studies asking about alternative occupations and constraints on choices might point to differences in the perceived alternatives. But these would be subject to the distorting effects of memory loss and dissonance reduction (rationalization). However, leads from such studies might then be followed up with slower, more expensive panels such as the youth panels currently being conducted by the Labor Department.

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Chapter 2

PERSISTENCE IN UNEMPLOYMENT AMONG ADULT MEN

Mary Corcoran and Martha S. Hill

INTRODUCTION

In this chapter we attempt to answer the question "Does initial unemployment increase the probability of later unemployment?" This question has important policy implications.[1] Phelps (1972) has argued that short-term economic policies that alleviate unemployment would lower long-run unemployment by preventing the loss of work-enhancing market experience. This argument assumes that past and future unemployment are causally related. Cripps and Tarling (1974) argued the reverse. They assumed that workers differ in their propensity for unemployment and that neither its incidence nor duration has a causal impact upon future unemployment. If this were true then short-run economic policies to alleviate unemployment (such as job creation programs) would have little long-run effect unless such programs explicitly attempt to reduce workers' propensities for unemployment.

Recent research on unemployment persistence has concentrated on teenage unemployment. Feldstein and Ellwood (1979), using the March 1976 Census Current Population Survey (CPS), examined the unemployment of men 16 to 19 years of age who were not in school. They reported that a small subset of this group of teenage men suffered most of the unemployment and that while their unemployment rate improved markedly as they moved into their twenties, it remained high relative to that of other young men. Ellwood (1979) found considerable persistence in nonemployment during the first four years following school completion among out-of-school teenage men who had 14 or fewer years of school. He also found that adjustments for differences in teenagers' propensity to work considerably reduced this persistence in nonemployment. Early nonemployment significantly lowered young men's future wages, even after corrections for heterogeneity. Corcoran (1979) has reported similar results based on a parallel analysis of teenage women's employment behavior. Meyer and Wise (1979) reported similar results for the employment behavior of a national sample of male high school graduates.

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[1] We are grateful to Heckman (1979) for pointing out this particular application of the heterogeneity-state dependence methodology.

It would be useful to know if the unemployment of teenagers and adult men follow the same patterns. There has been little analysis of persistence in the unemployment behavior of adult men, yet such men form a large proportion of the labor force and presumably would constitute a sizeable fraction of the recipients of any unemployment program. Proponents of dual labor market theories argue that worker behavior is altered (or reinforced) as a result of work conditions. In particular, they argue that men whose first jobs are unstable and fail to reward tenure, punctuality, and so forth may become alienated, develop poor work habits, and/or become less attached to the labor force (Piore, 1974; Cain, 1977). If so, we would expect to observe a small group of adult men who experience substantial and repeated unemployment. However, since these segmented labor market theories emphasize that early labor market experiences change workers' traits, persistence in adult men's unemployment behavior could very well be due to individual differences in their propensity to work. This is also consistent with other theoretical perspectives. Neoclassical economists, for instance, might argue that while for many people unemployment is an infrequent experience which results from the normal job sorting processes, there could be a small group who experience repeated employment instability because of characteristics (tastes for leisure, low skills, low motivations, etc.) which they brought to the labor market.

Sample

In this chapter we examine the persistence of unemployment among adult men using a sample of 1251 men aged 35-64 who were household heads and labor force participants[2] every year of the ten-year period 1968-1977.[3] Such prime age male workers traditionally have lower unemployment rates than other subgroups of

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[2] Individuals were considered labor force participants if they reported themselves working, temporarily laid-off, or unemployed/looking for work at the time of the interview.

[3] The sample was restricted to stable household heads because ten-year unemployment information was unavailable for other individuals. It was restricted to prime-age males because females and young males who were household heads over such a long period were likely to be quite unrepresentative of those who were household heads at the end of the period, and because many older males retired before the end of the long run observation period.

workers--the young and females--but constitute a large segment of the labor force and thus their unemployment is important in the aggregate. Further, a prime-age male household head is often the primary or only earner in his family, so those who are unemployed would presumably form an important part of any unemployment program.

Research Problem

There are at least four possible reasons why past unemployment could be associated with future unemployment. First, workers may differ in their propensity to work; those with high unemployment rates could be more weakly attached to the labor force than other workers because of personal preferences, motivations, or talents. Such individual differences in characteristics are referred to as "heterogeneity."

Another possibility is that past unemployment may directly increase the chances of present unemployment; that is, there may be a causal link between past and present unemployment. Such a link is often referred to as "state dependence." This might occur for several reasons. For example, unemployment might alter a worker's tastes, skills or motivations; or employers may judge a worker's potential by his past employment record so that workers with high instability may be tracked into less secure jobs.

A third possibility is that exogenous factors which influence unemployment and which are correlated over time may generate persistence in unemployment. Local demand conditions are one example of such exogenous factors. In such a case, past and present unemployment status would be correlated simply because both would be influenced by the tightness of the local labor market.

A fourth possibility is that unemployment persistence observed with available data may be an artifact of the data collection procedure. Most longitudinal surveys ask people about their unemployment over the past year. Even if there were no causal link between past and present unemployment, a single unemployment spell could overlap two adjacent years, yielding a spurious estimate of persistence.

The extent to which each of these four explanations accounts for the persistence in men's unemployment behavior has important implications both for our view of how the labor market operates and for government unemployment policies. This chapter estimates the extent and costs of unemployment persistence for prime-age men and attempts to disentangle effects of these four explanations of unemployment persistence. In the first section, we show that there is considerable persistence in the unemployment of adult men. Next, we describe the econometric techniques used to test the competing, though not mutually exclusive, explanations for this persistence. In the third section, we present an estimate of persistence in unemployment behavior which controls for constant differences across individuals ("stationary heterogeneity") that affect their individual likelihoods of unemployment. Controlling for these individual specific probabilities, we find that the odds that a man was unemployed one year given he had been unemployed in the previous year were 3.6 times higher than if he had experienced no unemployment in the previous year. This estimate of persistence picks up effects of serially correlated exogenous factors and effects of data collection procedures (i.e., overlap problems), as well as any causal relationship between past and present unemployment. Next we attempt to eliminate effects of overlap problems by asking whether, given a man's current unemployment status, his prior history provides information about his future. There was no evidence that this is the case. That is, we find no persistence in men's unemployment once we adjust for data collection procedures and for stationary heterogeneity. The final section summarizes the findings from our analyses and discusses the implications of our results.

EXTENT AND PERSISTENCE OF UNEMPLOYMENT

In any given year between 1967 and 1976, approximately one in ten of the men in our sample reported some unemployment. The probability of unemployment was fairly consistent across years, ranging from a low of 8.4 percent in 1972 to a high of 13.6 percent in 1974 (Table 2.1). Between 1967 and 1976, four out of ten men reported some unemployment. For those who were unemployed, the average time and money[4] costs of their unemployment were large in absolute terms, amounting to 27.3 weeks out of work and $5,113 in lost disposable income (Table 2.2). In relative terms, these costs translated to an average of 5.3 percent of expected ten-year work time and 4.2 percent of expected ten-year labor income in the absence of unemployment.

While average relative losses were low for most workers, a small proportion of workers experienced extremely high costs. Five percent of our sample accounted for almost half (46.6 percent) of the unemployment time of the entire sample. These men averaged time losses amounting to 96 weeks of work or 19.1 percent of their ten-year committed work time; they averaged earnings losses

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[4] All dollar values were inflated to the 1976 price level.

Table 2.1

PROBABILITY OF UNEMPLOYMENT BY YEAR, 1967-1976

	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976
Percent Unemployed	10.7	10.0	9.6	13.3	11.2	8.4	8.6	13.6	11.7	10.3

Table 2.2

TEN-YEAR UNEMPLOYMENT COSTS

Sample	N	Weighted Percent	Percent Unemployed	Mean Weeks Lost	Percent of Committed Work Time Lost	Mean Lost Disposable Income	Mean Expected Income Lost
All men	1251	100.0	38.4	10.5	2.0	$ 1,963	.016
Men with any unemployment	549	38.4	100.0	27.3	5.3	$ 5,113	.042
Percent of sample with most unemployment	75	5.1	100.0	96.0	19.1	$19,114	.154

amounting to $19,114 dollars in lost disposable income or 15.4 percent of their expected after-tax ten-year labor income.

Year by year observation of unemployment experiences documents the persistence of unemployment more clearly. Figure 2.1 is a probability tree for unemployment between 1972 and 1976.[5] The first branch corresponds to 1972 (year 1); the second to 1973 (year 2) and so on. A "1" indicates that a worker was unemployed at some time during that year; a "0" indicates he was never unemployed during that year. Above the line in any branch is the estimated probability of being in that state, conditional on following the previous branch. Below the

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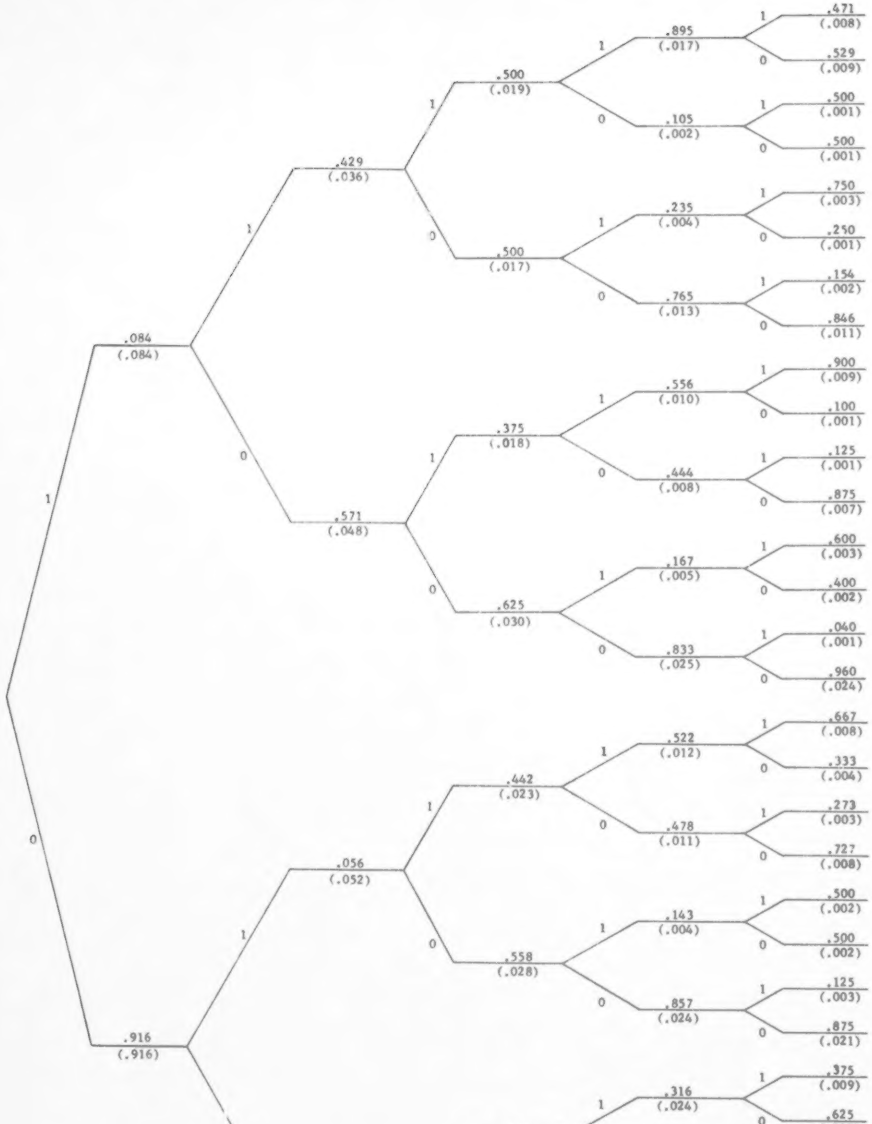
[5] We also calculated a probability tree for the years 1967 to 1971; results were much the same. We restricted this analysis to five years for ease of presentation.

Figure 2.1


1
0
.084
(.084)
.916
(.916)
1
0
1
0
.429
(.036)
.571
(.048)
.056
(.052)
.944
(.865)
1
0
1
0
1
0
1
0
.500
(.019)
.500
(.017)
.375
(.018)
.625
(.030)
.442
(.023)
.558
(.028)
.081
(.076)
.919
(.789)
1
0
1
0
1
0
1
0
1
0
1
0
1
0
1
0
.895
(.017)
.105
(.002)
.235
(.004)
.765
(.013)
.556
(.010)
.444
(.008)
.167
(.005)
.833
(.025)
.522
(.012)
.478
(.011)
.143
(.004)
.857
(.024)
.316
(.024)
.684
(.052)
.054
(.043)
.947
(.747)
1
0
.471
(.008)
.529
(.009)
.500
(.001)
.500
(.001)
.750
(.003)
.250
(.001)
.154
(.002)
.846
(.011)
.900
(.009)
.100
(.001)
.125
(.001)
.875
(.007)
.600
(.003)
.400
(.002)
.040
(.001)
.960
(.024)
.667
(.008)
.333
(.004)
.273
(.003)
.727
(.008)
.500
(.002)
.500
(.002)
.125
(.003)
.875
(.021)
.375
(.009)
.625
(.015)
.058
(.003)
.942
(.049)
.405
(.017)
.595
(.025)
.037
(.028)
.963
(.719)

line in parentheses is the estimated unconditional probability of being on that branch (or the proportion of all persons on that branch). Thus, 42.9 percent of the men who had been unemployed in 1972 (year 1) were also unemployed in 1973 (year 2) and 3.6 percent of all men were unemployed in both years.

This probability tree shows a strong association between past and present unemployment. A man was 7.7 times more likely to be unemployed in 1973 if he was unemployed in 1972 than if he had experienced no unemployment in 1972 (.429 versus .056). By 1976, a man who had been unemployed in each of the previous four years was 12.7 times more likely to report unemployment in 1976 than was a man who had never been unemployed from 1972 to 1975 (.471 versus .037).

These patterns can be misleading, however. Suppose for instance, that unemployment spells were ten weeks long and were distributed randomly throughout the year. In that case, about one-fifth of all the unemployment spells in one year would overlap into the next. Presumably, however, overlap problems would have less effect on the probabilities of unemployment in the years 1974, 1975 or 1976 conditional on unemployment in 1972 than on the probability of unemployment in 1973 conditional on unemployment in 1972. Table 2.3 suggests that unemployment in 1972 was a good predictor of later unemployment--even four years later. One out of three men who reported unemployment time in 1972 also reported some unemployment in 1976, but less than one in twelve of the men who reported no unemployment in 1972 experienced any in 1976.

Table 2.3

ESTIMATED PROBABILITY OF UNEMPLOYMENT IN YEARS 1973, 1974, 1975 AND 1976 CONDITIONAL ON EMPLOYMENT STATUS IN 1972

Employment Status in 1972	Conditional Probability of Unemployed			
	1973	1974	1975	1976
Unemployed	.429	.440	.429	.333
Never Unemployed	.056	.108	.091	.080

We also examined unemployment persistence using a simple correlation matrix. Table 2.4 gives the correlations for weeks of unemployment for the years 1972 to 1976. Again, we found evidence of considerable persistence in unemployment

Table 2.4

CROSS-YEAR CORRELATIONS BETWEEN WEEKS UNEMPLOYED 1967-1976

Weeks Unemployed	Weeks Unemployed									
	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976
1967	1.0000									
1968	0.4164	1.0000								
1969	0.2362	0.4437	1.0000							
1970	0.1967	0.3764	0.3076	1.0000						
1971	0.1873	0.2872	0.1567	0.4051	1.0000					
1972	0.1931	0.2320	0.1063	0.2499	0.3577	1.0000				
1973	0.1544	0.1474	0.1007	0.1833	0.1873	0.3613	1.0000			
1974	0.2062	0.2352	0.1499	0.2404	0.1507	0.2374	0.3200	1.00		
1975	0.2295	0.2590	0.1489	0.2755	0.2058	0.1562	0.2045	0.3355	1.0000	
1976	0.1261	0.2185	0.1281	0.3091	0.1637	0.1541	0.2245	0.3584	0.4722	1.0000

behavior. Adjacent year correlations in weeks unemployed ($r_{t,t+1}$) were quite high--ranging from .31 ($r_{69,70}$) to .47 ($r_{75,76}$). Two years later, cross-year correlations ($r_{t,t+3}$) typically hovered at or below .20. After this, correlations dropped only slightly so that, for instance, the correlation between hours unemployed in the years 1967 and 1976 was .13 and that between hours unemployed in 1968 and 1976 was .22. This represented considerably more persistence than would be expected from simple overlap problems. Underlying differences in worker characteristics might be expected to account for at least part of this persistence.

There was a great deal of stability in the unemployment patterns of adult male household heads over the 1972-1976 period. Unemployment did not appear to have been randomly distributed in the population. A small proportion of men with protracted unemployment bore very high time and money costs. In the next sections of this chapter we discuss the question: "Does persistence in unemployment reflect individual differences in workers' characteristics such as motivations and abilities, or is future likelihood of unemployment causally related to past unemployment?"

ECONOMETRIC MODELS FOR EXPLORING SOURCES OF PERSISTENCE IN MEN'S UNEMPLOYMENT

Apparent persistence in adult men's unemployment could be due to several, quite distinct factors--heterogeneity, a causal impact of past unemployment on future employment stability, serially correlated exogenous factors, or data collection procedures. We attempted to isolate the effects of such factors using techniques developed by Chamberlain (1978b, 1978c, 1979) and Anderson (1973) for use with sequences of binary unemployment information. These techniques allowed us to estimate measures of association between past and present unemployment while eliminating any association due either to stationary heterogeneity or to data collection procedures. First, let us consider the following autoregressive logistic model which allows for stationary heterogeneity (constant differences across individuals that affect the likelihood that they would become unemployed):

$$(1)\quad \mathrm{Prob}(y_{it}=1|y_{i,t-1}) = \frac{\exp(\alpha_i + \gamma y_{i,t-1})}{1 + \exp(\alpha_i + \gamma y_{i,t-1})}$$

where y_{it} = 1 if unemployed in year t, otherwise

α_i = personal characteristics that influence an individual's probability of unemployment

γ = the degree to which unemployment last year affects the probability of unemployment this year.

This equation states that the conditional probability that a man is unemployed in year t, given his unemployment status in the previous year--Prob $(y_{it}=1|y_{i,t-1})$--depends upon (1) a set of personal characteristics, α_i, which remain constant for that individual and have the same effect on the likelihood of unemployment over the period considered, and (2) whether he was unemployed in the previous year, $(y_{i,t-1})$. If these personal characteristics completely explained unemployment persistence, then γ would be equal to zero. This can be tested by constructing a confidence interval for γ.

Four features of this model should be noted. First, α_i does not vary over time; that is, we assume that those individual-specific personal traits which influence the conditional probabilities of unemployment were fixed for the period considered. This assumption was thought to be not too unreasonable for our sample of adult male household heads with a ten-year record of labor force participation. Second, there are no X's specified in this model. Effects of constant X's are captured in α_i; effects of changing X's which are correlated over time are captured in γ. Thus, this model does not allow us to separate persistence that is due to a causal relationship between past and present unemployment from persistence that is due to exogenous factors that are correlated over time (such as local demand conditions). Third, γ is assumed to be constant over time and over individuals; that is, the effect of one year's unemployment on the next year's unemployment does not change over time and is constant for individuals. Since we concentrated the analysis on a five year period (1972 to 1976), this assumption did not appear too unreasonable for our sample of adult men. Finally, this model makes no adjustment for the way unemployment data is collected and recorded; thus our estimate of γ would pick up any observed persistence in unemployment due to data collection procedures.

The next issue was to solve this model to get a consistent estimator of γ. Maximizing the joint likelihood function over α_i and γ will not provide consistent estimates. The number of individual-specific parameters, α_i, to be estimated in the nonlinear form increases as the sample size increases, so that increasing sample size does not produce desired asymptotic properties. Chamberlain showed, however, that it is possible to get a consistent estimator of γ if we use a conditional likelihood function. The basic idea is that the number of years a man is unemployed over the period ($S_i = \sum_{i=1}^{T} y_{it}$) and his unemployment status for the final year of observation (y_{iT}) provide sufficient statistics for the omitted constant individual-specific factors, α_i. That is, men with the same values on S_i and y_{iT} have the same expected value on α_i. When S_i and y_{iT} are held fixed, this assumption allows α_i to drop from the likelihood function. [illegible] deal with initial conditions by assuming that men must have identical values for y_{i1} in order to have identical individual-specific parameters, α_i. For this, we can use Chamberlain's conditional likelihood function to estimate γ.[6] Appendix 2.1 describes in some detail the derivation of this conditional function and the estimation procedure.

Suppose that γ differs significantly from zero--that is, that past unemployment predicts future unemployment--even after adjusting for individual differences which affect a man's likelihood of unemployment in both periods. This finding would not allow us to conclude that a man's past and current employment are causally related because data collection and coding procedures may have generated spurious association between past and current unemployment.[7] That is, γ could be nonzero simply because an unemployment spell could span two years. If, for instance, all unemployment spells lasted ten weeks and were randomly distributed then about one-fifth would overlap across two years.

To describe a man's unemployment history completely we would want to know the length and timing of all spells of work and unemployment. If his history did not help us to predict his future, given his current state, then this is a Markov process. Chamberlain (1978) termed deviations from the Markov property "duration dependence." He pointed out that duration independence would imply that a man's unemployment history prior to the current spell should not affect the distribution of the length of the current spell; and the amount of time spent in the current spell should not affect the distribution of remaining time in that spell. This implies that the duration of the spells would be independent of each other and the distribution of time in a state would be exponential.[8] If we assumed that all spells of unemployment had the same distribution, that all spells of work had the same distribution, and that the exponential rate parameter for each of the states were the same for all spells, then we would have an alternating Poisson process. In this case, the stationary heterogeneity model implies that each man's unemployment transition probabilities are characterized by the two parameters of an alternating Poisson process.[9] Departure from this model would be evidence of correlation between past and current chances of unemployment at the individual level; that is, even given his current state, a man's past history would help predict his future.

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[6] This affects the distribution of $\underline{\alpha}$, but since we are conditioning on $\underline{\alpha}$, no problems arise.

[7] Note also that we have not taken into account effects of serially correlated exogenous factors that influence men's chances of becoming unemployed.

[8] Note that this definition of duration dependence is broader than that used by Heckman and Borjas (1979) and that used by Chamberlain (1979). We define duration dependence as occurring if spells of unemployment are not independent or if the amount of time spent in the current spell affects the distribution of remaining time in that spell. Chamberlain (1979) and Heckman and Borjas(1979) define "duration dependence" as the second of these two possibilities. Given that we are dealing with binary unemployment histories and that we have no data on the durations of unemployment spells, we cannot estimate their narrower definition of duration dependence.

We could test for duration dependence using binary sequences of information on unemployment states, but the techniques would vary depending upon whether the data were generated by point or by interval sampling. In point sampling, men are asked to report whether they are currently unemployed; in interval sampling men report whether they were unemployed at any time during a recent period. With point estimates of states, the technique is quite straightforward. If there is no duration dependence, then the observed binary sequences should correspond to a first order Markov chain. Thus, the test for state dependence reduces to a test for second (or higher) order dependence after allowing each man to have his own first order Markov chain.

For interval sampling, the test for duration dependence is more complex (Chamberlain, 1978c). The basic underlying this test is that stationary heterogeneity implies that a man's probability of being unemployed in period t depends upon the number of consecutive periods preceding period t during which he was unemployed. The reasoning goes as follows. If a man's unemployment is characterized by an alternating Poisson process then only his unemployment state at the end of the preceding year is relevant to his status in any one year. If $y_{t-1}=1$, we know only that he was unemployed sometime during the preceding year. We do not know whether he was unemployed at the end of the preceding year. In this case, y_{t-2} could affect the probability that he was unemployed early in year t-1 rather than late in that year; he is more likely to have been unemployed in year t-1 if $y_{t-2} = 1$ than if $y_{t-2} = 0$, because of the possibility of one unemployment spell spanning both years. However, if $y_{t-1} = 0$, then the man was never unemployed the previous year and thus was not unemployed at the end of that year. In this case, since we know his state at the end of year t-1, his state in years t-2, t-3,...1 would be irrelevant if his unemployment/employment process were indeed following an alternating Poisson process. This implies:

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[9] This is a very brief summary of an argument developed by Chamberlain in a series of papers. See Chamberlain (1978b, 1978c) for a more extensive coverage of these points.

$$(2)\quad \mathrm{Prob}(y_t=1|y_{t-1},y_{t-2},\ldots) = \mathrm{Prob}(y_t=1|y_{t-1} = \ldots = y_{t-J} = 1, y_{t-J-1} = 0)$$

$$= \mathrm{Prob}(y_t=1|J)$$

where J = the number of consecutive preceding years that the man was unemployed. That is, assuming no duration dependence and assuming an alternating Poisson process, the probability that a man is unemployed in year t depends only on how many consecutive years he was unemployed in the years immediately preceding year t. This would give the following logistic model:

$$(3)\quad \mathrm{Prob}(y_{it}=1|y_{i,t-1},y_{i,t-2},\ldots) = \frac{\exp\ (A_i)}{1 + \exp(A_i)}$$

$$\text{where } A_i = \alpha_i + \sum_{k=1}^{\infty} \Psi_{ik} \prod_{j=1}^{k} y_{i,t-j}.$$

Here, each man has his own set of parameters α_i and Ψ_{ik} which determine the nature of his Poisson distributions for time in employment and unemployment spells. Chamberlain extended this model to test for duration dependence as follows:

$$(4)\quad \mathrm{Prob}(y_{it} = 1|y_{i,t-1}, y_{i,t-2}, \ldots) = \frac{\exp(A_i + \gamma_2 y_{i,t-2})}{1 + \exp(A_i + \gamma_2 y_{i,t-2})}$$

The test is based on the magnitude and significance of γ_2. If γ_2 is not significantly different from zero then we can conclude that there is no duration dependence. That is, we can conclude that given a man's current unemployment status, his past unemployment history has no power in predicting his future likelihood of unemployment. Chamberlain has shown that for $T \geq 6$ and large N, we can consistently estimate γ_2 using a conditional likelihood function. This function and estimation procedure are described in Appendix 2.3.

EMPIRICAL RESULTS: SOURCES OF PERSISTENCE IN UNEMPLOYMENT INCIDENCE

The techniques discussed in the previous section were used to analyze the persistence of unemployment. We began by obtaining an estimate of first-order dependence that was based on men's binary unemployment sequences for the years 1972 to 1976 using Chamberlain's autogressive logistic model, where each man is assigned his own unemployment probability (Equation 1). This yields $\hat{\gamma}$ = 1.29

with a standard error of .36. This estimate is based on the unemployment sequences of 131 men (9.1 percent of the sample). Appendix 2.2 describes exactly how we calculated this estimate of γ.

Since $\hat{\gamma}$ is significantly different from zero at the .01 level of confidence, we can tentatively conclude that a man's unemployment in one year is a good predictor of his unemployment in the next year allowing for stationary heterogeneity. Our estimate implies that given that a man was unemployed in the previous year, the odds that the same man is unemployed, are $e^{1.29} = 3.6$ times higher than if he was not unemployed last year. While high, these odds are much lower than the odds we found when we ignored heterogeneity. Not allowing for unobserved person factors that have constant effects on a worker's chances of becoming unemployed increased these odds to 12.1.[10]

Applying Chamberlain's model to unemployment sequences for the period 1967 to 1971 gave much the same results. The estimate of first order dependence was smaller but still significant ($\hat{\gamma}$ =.86 with a standard error of .30) and was within two standard deviations of the estimate for 1972 to 1976. This again suggests that past unemployment significantly predicts future unemployment, even after adjustments for heterogeneity.

Recall however, that we would expect to observe some persistence in men's unemployment behavior simply because of the way in which unemployment information is recorded. That is, past and current unemployment can be associated simply because a single unemployment spell may span two years. Instead of asking whether a man's previous year's unemployment status helps predict his current year's unemployment status, we may want to ask, given his current unemployment status, whether his prior unemployment history would enable us to predict his future chances of unemployment. That is, does unemployment behavior deviate from a Markov process? Departures from the Markov property are evidence of duration dependence--evidence that, given a man's current unemployment status, his prior unemployment history is informative about his future likelihood of becoming unemployed.

We used information on men's eight-year unemployment sequences for the years 1969 to 1976 in conjunction with Chamberlain's procedure to assess the magnitude and significance of duration dependence (techniques based on Equation 4). The basic idea underlying this procedure is that stationary heterogeneity, in the absence of duration dependence, implies that the likelihood that a man is unemployed in year t will depend only upon how many consecutive years he was unemployed in the years immediately preceding year t.

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[10] We calculate these odds as follows. For each year after the first, we calculate the probability that a man was unemployed, given he was unemployed in the previous year. The average of the probabilities over years 2 to T is equal to the average value of $P(1|1)$. Similarly, we calculate the average value of $P(1|0)$. For five years, these average values are .467 and .068. The odds are: $e^{\gamma} =(.467/.533)X(.932/.068)=12.1$. Here γ would equal 2.5.

There were 91 men with eight-year unemployment sequences that generated conditional probabilities that allowed us to obtain an estimate of duration dependence. Applying Chamberlain's model to the eight year unemployment sequences gives an estimate of $\gamma_2 = -.25$ with a standard error of .43.[11] This point estimate is quite close to zero and has a large standard error. Thus, it seems that given an individual's current unemployment status, his prior unemployment history is not informative about his future. This strongly suggests that past unemployment does not increase adult men's chances of current unemployment. Of course, we have not eliminated effects of serially correlated exogenous factors that are changing over time and that affect the likelihood of unemployment. Perhaps effects of such exogenous factors are cancelling out effects of a causal link. If this were the case, then it may be that past unemployment actually reduces the chances of subsequent unemployment.

SUMMARY AND CONCLUSIONS

Our results may be summarized as follows:

1) There was considerable inequality in the distribution of long-run unemployment among adult male household heads. Five percent of male household heads accounted for almost half the unemployment time of male heads between 1967 and 1976.
2) This five percent suffered considerable losses as a result of this unemployment. Work time losses averaged 96 weeks and income losses averaged $19,114.
3) There was considerable persistence in unemployment behavior. Men with poor records early on were more likely to have poor records later.
4) A large part of this persistence can be attributed to unmeasured differences in workers' propensity for unemployment. The odds that a man was unemployed in 1976 given that he was unemployed in 1972 were 12.1 times higher than if he experienced no unemployment in 1972. These odds

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[11] Use of ten years of data gives a similar estimate of .20 with a standard error of .39, based on 189 cases.

dropped considerably from 12.1 to 3.6 when adjustments were made for heterogeneity.

5) Given a man's current employment status, his earlier history of unemployment does not predict future unemployment. This suggests that the observed persistence in unemployment behavior is a function of unmeasured individual differences and data collection procedures. There was no evidence of a causal link between past and current unemployment.

Results did not support the hypothesis that unemployment after age 25 raises men's chances of future unemployment. Apparently data collection procedures and unmeasured personal differences in the propensity for unemployment generated the considerable persistence in observed unemployment. Unless job creation programs also attempt to identify and alter those skills, attitudes, or habits which influence work stability, they will not lower future unemployment. Job creation programs may serve other needs, however. One must also consider how unemployment affects the lifetime earnings of adult men and the short-run economic well-being of their families when evaluating job creation policies. Ellwood (1979), Meyer and Wise (1979), and Corcoran (1979) have shown that periods of non-work in the teenage years reduce later wages. This may hold true for adult men as well. Providing work may forestall this drop in lifetime earnings power. Also, at least in the short run, jobs policies may provide sorely needed economic relief for families hit by unemployment.

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Appendix 2.1

DERIVATION AND ESTIMATION OF γ USING A CONDITIONAL LIKELIHOOD FUNCTION

Derivation

Recall Equation 1:

$$\text{Prob } (y_{it} = 1 \mid y_{i,t-1}) = \frac{\exp\ (\alpha_i + \gamma y_{i,t-1})}{1 + \exp\ (\alpha_i + \gamma y_{i,t-1})}$$

Since the number of individual-specific parameters, α_i, increases as the sample size increases, we cannot derive consistent estimates of both α_i and γ using a joint likelihood function. Fortunately, Chamberlain has developed a technique using a conditional likelihood function which allows us to obtain a consistent estimate of γ; this technique eliminates α_i from the model. Use of this technique involves the fairly reasonable assumption that the expected values of α_i are identical for men with certain patterns of experience.

Equation 1 implies that the probability that a man experiences his observed binary pattern of unemployment over the period t=1, ... , T is given by:

$$\text{(1a)} \quad \text{Prob } (y_{i1} \ldots y_{iT}) = \text{Prob } (y_{i1}) \cdot \prod_{t=2}^{T} \text{Prob } (y_{it} \mid y_{i,t-1}) \ .$$

Since for $T \leq t < 1$

$$\text{Prob } (y_{it} = 1 \mid y_{i,t-1}) = \frac{\exp\ (\alpha_i + \gamma\ y_{t-1})}{1 + \exp\ (\alpha_i + \gamma\ y_{t-1})}$$

and

$$\text{Prob } (y_{it} = 0 \mid y_{i,t-1}) = \frac{1}{1 + \exp\ (\alpha_i + \gamma\ y_{t-1})} \ ,$$

Equation 1a translates to

$$(2)\ \text{Prob}\ (y_{i1} \ldots y_{iT}) =$$

$$\text{Prob}\ (y_{i1}) \cdot \left(\frac{\exp\ (\alpha_i)^{\sum_{t=2}^{T} y_{it}} \quad \exp\ (\gamma)^{\sum_{t=2}^{T} y_{it} \cdot y_{i,t-1}}}{(1 + \exp\ (\alpha_i + \gamma))^{\sum_{t=2}^{T} y_{i,t-1}} \quad (1 + \exp\ (\alpha_i))^{T - 1 - \sum_{t=2}^{T} y_{i,t-1}}} \right) .$$

This is because for $T \leq t < 1$ the numerator is multiplied by:

$\exp\ (\alpha_i)$ when $y_{it} = 1$

$\exp\ (\gamma\ y_{t-1})$ when both $y_{it} = 1$ and $y_{i,t-1} = 1$,

and the denominator is multiplied by:

$1 + \exp\ (\alpha_i)$ when $y_{i,t-1} = 0$

$1 + \exp\ (\alpha_i + \gamma)$ when $y_{i,t-1} = 1$.

All terms containing an α_i can be combined:

$$(3)\ \text{Prob}\ (y_i \ldots y_{iT}) = \theta_i \cdot \exp\ (\gamma)^{\sum_{t=2}^{T} y_{it} \cdot y_{i,t-1}} \cdot \text{Prob}\ (y_1)$$

$$\text{where } \theta_i = \frac{\exp\ (\alpha_i)^{\sum_{t=2}^{T} y_{it}}}{(1 + \exp\ (\alpha_i + \gamma))^{\sum_{t=2}^{T} y_{i,t-1}} \quad (1 + \exp\ (\alpha_i))^{T - 1 - \sum_{t=2}^{T} y_{i,t-1}}} .$$

Since $\sum_{t=2}^{T} y_{it} = (\sum_{t=1}^{T} y_{it}) - y_{i1}$

and $\sum_{t=2}^{T} y_{i,t-1} = (\sum_{t=1}^{T} y_{it}) - y_{iT}$,

we know that θ_i depends only on α_i, γ, y_{i1}, $\sum_{t=1}^{T} y_{it}$, and y_{iT} . In fact, individuals with the same expected value for α_i, y_{i1}, $\sum_{t=1}^{T} y_{it}$, and y_{iT} have identical values for θ_i. Chamberlain's technique takes advantage of this in order to drop out all terms containing α_i and thus reduce the unknowns to γ only. This technique involves computing Prob $\left((y_{i1} \ldots y_{iT}) \mid y_{i1}, y_{iT}, \sum_{t=1}^{T} y_{it}\right)$ on the assumption that all individuals with the same initial and end state (y_{i1} and y_{iT}, respectively) and the same number of years of unemployment ($\sum_{t=1}^{T} y_{it}$) have identical expected values for the effects of their personal characteristics on their unemployment transition probabilities (α_i). If this is true, then

$$(4)\ \text{Prob}\left((y_{i1} \ldots y_{iT}) \mid y_{i1}, y_{iT}, \sum_{t=1}^{T} y_{it}\right) = \frac{\text{Prob}\ (y_{i1} \ldots y_{iT})}{\sum\limits_{d \in B_i} \text{Prob}\ (d_i \ldots d_T)}$$

where $B_i = \left\{ \underset{\sim}{d} = (d_1 \ldots d_T) \mid d_t = 0 \text{ or } 1,\ d_1 = y_{i1},\ d_T = y_{iT},\ \sum_{t=1}^{T} d_t = \sum_{t=1}^{T} y_{it} \right\}$.

This means:

$$(5)\ \text{Prob}\left((y_{it} \ldots y_{iT}) \mid y_{i1}, y_{iT}, \sum_{t=1}^{T} y_{it}\right)$$

$$= \frac{\Theta_i \exp(\gamma)^{\sum_{i=1}^{T} y_{it} y_{i,t-1}} \cdot \text{Prob}\ (y_1)}{\sum\limits_{d \in B_i} \Theta_i \exp(\gamma)^{\sum_{i=1}^{T} d_t d_{t-1}} \cdot \text{Prob}\ (y_1)}$$

This equation then reduces to:

$$(6)\ \text{Prob}\left((y_{it}\cdots y_{iT})\ \middle|\ y_{i1},\ y_{iT},\ \sum_{t=1}^{T} y_{it}\right) = \frac{\exp(\gamma)^{\sum_{i=1}^{T} y_{it}\, y_{i,t-1}}}{\sum_{d\varepsilon B_i} \exp(\gamma)^{\sum_{i=1}^{T} d_t\, d_{t-1}}}$$

Thus, this conditional probability yields an expression which depends on only one unknown--γ.

Estimation of the Conditional Likelihood Function

To estimate γ from this formulation, we need to first isolate all of the possible sequences of states which retain a γ term as part of their conditional probability (for many, the γ terms in the denominator and numerator will cancel out). The next step is to estimate γ using information about the observed frequencies of this entire subset of sequences that depend on γ. The task in calculating an estimate of γ involves manipulating the conditional probabilities that depend on γ in such a manner as to obtain a formula that reduces to γ and uses information on all conditional probabilities that depend on γ.

Data on four or more periods are necessary to estimate γ; with three or fewer periods the conditional probabilities for all possible sequences equal one. The difficulty of the task of estimating γ increases with the number of periods of observation since the number of possible sequences equals 2^j, where j is the number of observation periods.

For four periods, the procedure is relatively simple. This is because the restrictions allow a maximum of two sequences with the same beginning and end state and same total number of periods with a "positive" state (the only option left after these restrictions is the order of the states associated with the two middle periods). Additionally, with a four-period observation span, only two sets of restrictions result in conditional probabilities that retain γ as part of the probability--$\{y_1 = 0, y_4 = 1, \sum_{t=1}^{4} y_t = 2\}$ and $\{y_1 = 1, y_4 = 0, \sum_{t=1}^{4} y_t = 2\}$. For the first set:

$$\text{Prob } [(0011) \mid y_1 = 0, y_4 = 1, \sum_{t=1}^{4} y_t = 2] =$$

$$\frac{\exp\ (\gamma\ ((0\cdot 0) + (1\cdot 0) + (1\cdot 1)))}{\exp\ (\gamma\ ((0\cdot 0) + (1\cdot 0) + (1\cdot 1))) + \exp\ (\gamma\ ((1\cdot 0) + (0\cdot 1)\ (1\cdot 0)))} = \frac{\exp \gamma}{\exp \gamma + 1}$$

and

$$\text{Prob } [(0101) \mid y_1 = 0, y_4 = 1, \sum_{t=1}^{4} y_t = 2] =$$

$$\frac{\exp\ (\gamma\ ((1\cdot 0\ ) + (0\cdot 1) + (1\cdot 0)))}{\exp\ (\gamma\ ((0\cdot 0) + (1\cdot 0) + (1\cdot 1))) + \exp\ (\gamma\ ((1\cdot 0) + (0\cdot 1) + 1\cdot 0)))} = \frac{1}{\exp \gamma + 1} .$$

For the second set:

$$\text{Prob } [(1100) \mid y_1 = 1, y_4 = 0, \sum_{t=1}^{4} y_t = 2] = \frac{\exp\ (\gamma)}{1 + \exp\ (\gamma)}$$

and

$$\text{Prob } [(1010) \mid y_1 = 1, y_4 = 0, \sum_{t=1}^{4} y_t = 2] = \frac{1}{1 + \exp\ (\gamma)} .$$

Thus, we have 4 (out of a possible 16) conditional probabilities that retain γ terms. The frequencies, f, attached to all 4 of these probabilities should be used to calculate γ. Since

$$E\left(\frac{\text{Prob } [(0011) \mid y_1 = 0, y_4 = 1, \sum_{t=1}^{4} y_t = 2]}{\text{Prob } [(0101) \mid y_1 = 0, y_4 = 1, \sum_{t=1}^{4} y_t = 2]}\right) = \frac{\dfrac{f_{0011}}{f_{0011} + f_{0101}}}{\dfrac{f_{0101}}{f_{0011} + f_{0101}}} = \frac{f_{0011}}{f_{0101}}$$

and

$$E\left(\frac{\text{Prob }[(1100)\mid y_1 = 1,\ y_4 = 0,\ \sum_{t=1}^{4} y_t = 2]}{\text{Prob }[(1010)\mid y_1 = 1,\ y_4 = 0,\ \sum_{t=1}^{4} y_t = 2]}\right) = \frac{\dfrac{f_{1100}}{f_{1100} + f_{1010}}}{\dfrac{f_{1010}}{f_{1100} + f_{1010}}} = \frac{f_{1100}}{f_{1010}} ,$$

we have two estimates of γ:

$$\frac{f_{0011}}{f_{0101}} = \exp(\hat{\gamma}_1) \quad \text{and} \quad \frac{f_{1100}}{f_{1010}} = \exp(\hat{\gamma}_2) .$$

We can then pool these to get a final estimate of γ. Since we want to get the tightest estimate of γ, we compute the weighted average of $\hat{\gamma}_1$ and $\hat{\gamma}_2$, weighting by the inverse of their variances.

Appendix 2.2

ESTIMATING γ USING FIVE-YEAR UNEMPLOYMENT SEQUENCES

When T=5, there are six sets of restrictions on $\sum_t y_t$, y_1, and y_5 which produce conditional probabilities which depend on γ: These sets of restrictions are:

(1) $\{y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 2\}$

(2) $\{y_1 = 0,\ y_5 = 1,\ \sum_t y_t = 2\}$

(3) $\{y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 3\}$

(4) $\{y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 3\}$

(5) $\{y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 2\}$

(6) $\{y_1 = 1,\ y_5 = 1,\ \sum_t y_t = 3\}$

For every one of these six sets of restrictions there is a set of three corresponding sequences:

$\{y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 2\}$	(1,1,0,0,0), (1,0,0,1,0), and (1,0,1,0,0)
$\{y_1 = 0,\ y_5 = 1,\ \sum_t y_t = 2\}$	(0,0,0,1,1), (0,1,0,0,1), and (0,0,1,0,1)
$\{y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 3\}$	(1,1,1,0,0), (1,1,0,1,0), and (1,0,1,1,0)
$\{y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 3\}$	(0,0,1,1,1), (0,1,1,0,1), and (0,1,0,1,1)
$\{y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 2\}$	(0,1,1,0,0), (0,0,1,1,0), and (0,1,0,1,0)
$\{y_1 = 1,\ y_5 = 1,\ \sum_t y_t = 3\}$	(1,1,0,0,1), (1,0,0,1,1), and (1,0,1,0,1)

Thus, we have 18 sequences to base the estimate of γ on. As it turns out, four of the six sets of restrictions (1)-(4), yield conditional probabilities that involve the same specification, and the remaining two, (5)-(6), also yield conditional probabilities involving the same specification, but one that differs from that of restrictions (1)-(4).

1. $B = \{\underset{\sim}{y} \mid y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 2\}$

$$\text{Prob }(1,1,0,0,0 \mid B) = \frac{\exp(\gamma)}{\exp(\gamma) + 2}$$

$$\text{Prob }(1,0,0,1,0 \mid B) = \frac{1}{\exp(\gamma) + 2} = \text{Prob }(1,0,1,0,0 \mid B)$$

2. $B = \{\underset{\sim}{y} \mid y_1 = 0,\ y_5 = 1,\ \sum_t y_t = 2\}$

$$\text{Prob }(0,0,0,1,1 \mid B) = \frac{\exp(\gamma)}{\exp(\gamma) + 2}$$

$$\text{Prob }(0,1,0,0,1 \mid B) = \frac{1}{\exp(\gamma) + 2} = \text{Prob }(0,0,1,0,1 \mid B)$$

3. $B = \{\underset{\sim}{y} \mid y_1 = 1,\ y_5 = 0,\ \sum_t y_t = 3\}$

$$\text{Prob }(1,1,1,0,0 \mid B) = \frac{\exp(\gamma)}{\exp(\gamma) + 2}$$

$$\text{Prob }(1,1,0,1,0 \mid B) = \frac{1}{\exp(\gamma) + 2} = \text{Prob }(1,0,1,1,0 \mid B)$$

4. $B = \{\underset{\sim}{y} \mid y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 3\}$

$$\text{Prob}\ (0,0,1,1,1 \mid B) = \frac{\exp\ (\gamma)}{\exp\ (\gamma) + 2}$$

$$\text{Prob}\ (0,1,1,0,1 \mid B) = \frac{1}{\exp\ (\gamma) + 2} = \text{Prob}\ (0,1,0,1,1 \mid B)$$

5. $B = \{\underset{\sim}{y} \mid y_1 = 0,\ y_5 = 0,\ \sum_t y_t = 2\}$

$$\text{Prob}\ (0,1,1,0,0 \mid B) = \frac{\exp\ (\gamma)}{2\ \exp\ (\gamma) + 1} = \text{Prob}\ (0,0,1,1,0 \mid B)$$

$$\text{Prob}\ (0,1,0,1,0 \mid B) = \frac{1}{2\ \exp\ (\gamma) + 1}$$

6. $B = \{\underset{\sim}{y} \mid y_1 = 1,\ y_5 = 1,\ \sum_t y_t = 3\}$

$$\text{Prob}\ (1,1,0,0,1 \mid B) = \frac{\exp\ (\gamma)}{2\ \exp\ (\gamma) + 1} = \text{Prob}\ (1,0,0,1,1 \mid B)$$

$$\text{Prob}\ (1,0,1,0,1 \mid B) = \frac{1}{2\ \exp\ (\gamma) + 1}$$

Thus, with the first four sets of restrictions, we get one sequence with a conditional probability equal to $\frac{\exp\ (\gamma)}{\exp\ (\gamma) + 2}$ and two with a conditional probability equal to $\frac{1}{\exp\ (\gamma) + 2}$. With the last two sets of restrictions, we get two sequences with a conditional probability equal to $\frac{\exp\ (\gamma)}{2\ \exp\ (\gamma) + 1}$ and one sequence with a conditional probability equal to $\frac{1}{2\ \exp\ (\gamma) + 1}$.

We want to pool all of this information in deriving $\hat{\gamma}$. So what we can do is derive separate estimates for the two distinct categories of sequences, then take the weighted average of these two estimates, weighting by the inverse of their variances. To do this, we must have a formula for each category that uses all of the observed frequencies that fall into the given category and reduces to $\hat{\Upsilon}$. For the first category (A) with restrictions (1)-(4), this formula is:

$$\hat{\Upsilon}_A = \ln \frac{2\hat{\Psi}}{1-\hat{\Psi}}, \text{ where } \hat{\Psi} = \frac{R_1 + R_2 + R_3 + R_4}{N_1 + N_2 + N_3 + N_4}$$

Here R_i is the number of people in the sample with the sequence that yields a conditional probability equal to $\frac{\exp(\Upsilon)}{\exp(\Upsilon) + 2}$ in the i^{th} set of sequences in the (A) category and N_i is the sum of frequencies across all sequences in the i^{th} set of sequences. For category (B) the formula is:

$$\hat{\Upsilon}_B = \ln \frac{\hat{\tau}}{2(1-\hat{\tau})}, \text{ where } \hat{\tau} = \frac{R'_1 + R'_2}{N'_1 + N'_2}$$

Here, R'_i is the sum of the frequencies of the two sequences that yields a conditional probability equal to $\frac{\exp(\Upsilon)}{2\exp(\Upsilon) + 1}$ in the i^{th} set of sequences in the (B) category.[1]

After calculating $\hat{\Upsilon}_A$ and $\hat{\Upsilon}_B$ and their respective variances,[2] the final estimate of Υ is obtained by averaging $\hat{\Upsilon}_A$ and $\hat{\Upsilon}_B$, weighting by the inverse of their respective variances.

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[1] Within each set of sequences an $\hat{\Upsilon}$ can be calculated using the same formula but omitting all terms not specific to that given set of sequences.

[2] The formula for the variance for category (A) is:

$$V(\hat{\Upsilon}_A) = [(N_1 + N_2 + N_3 + N_4)\hat{\Psi}(1-\hat{\Psi})]^{-1},$$

and the formula for the variance for category (B) is:

$$V(\hat{\Upsilon}_B) = [(N'_1 + N'_2)\hat{\tau}(1-\hat{\tau})].$$

In our data we observe the following frequencies for the relevant 18 sequences based on a sample size of 1,251:

Sequence	Frequency (Proportion of Sample)
11000	.011
10010	.002
10100	.007
	.020
00011	.017
01001	.003
00101	.003
	.023
11100	.001
11010	.001
10110	.001
	.003
00111	.009
01101	.003
01011	.002
	.014
01100	.008
00110	.015
01010	.002
	.025
11001	.002
10011	.003
10101	.001
	.006

Thus,

$$\hat{\Psi} = \frac{(.011 + .017 + .001 + .009) \times (1251)}{(.020 + .023 + .003 + .014) \times (1251)} = .633 \text{ and}$$

$$\hat{\gamma}_A = \ln \frac{2\ \hat{\Psi}}{1 - \hat{\Psi}} = \ln \frac{2\ (.633)}{1 - .633} = 1.24 \text{ and}$$

$$\text{var } (\hat{\gamma}_A) = ((1251)\ (.020 + .023 + .003 + .014)\ \hat{\Psi}\ (1 - \hat{\Psi})^{-1}) = .057.$$

And $\hat{\tau} = \dfrac{(.008 + .015 + .002 + .003) \times (1251)}{(.025 + .006) \times (1251)} = .903$ and

$$\hat{\gamma}_B = \ln \frac{\hat{\tau}}{2\ (1 - \hat{\tau})} = \ln \frac{.903}{2\ (1 - .903)} = 1.54 \text{ and}$$

$$\text{var } (\hat{\gamma}_B) = (1251\ (.025 + .006)\ \hat{\tau}\ (1 - \hat{\tau}))^{-1} = .295$$

For our final estimate of γ we calculate the average of $\hat{\gamma}_A$ and $\hat{\gamma}_B$, weighting by the inverse of their respective variances and standardizing for these weights:

$$\hat{\gamma} = \frac{\hat{\gamma}_A\ (\text{var } (\hat{\gamma}_A))^{-1} + \hat{\gamma}_B\ (\text{var } (\hat{\gamma}_B))^{-1}}{\text{var } (\hat{\gamma}_A)^{-1} + \text{var } (\hat{\gamma}_B)^{-1}} = \frac{1.24\ (\frac{1}{.057}) + 1.54\ (\frac{1}{.295})}{\frac{1}{.057} + \frac{1}{.295}} = 1.29.$$

And the variance of $\hat{\gamma}$ is:

$$\text{var } (\hat{\gamma}) = \frac{\text{var } (\hat{\gamma}_A)\left(\dfrac{\text{var } (\hat{\gamma}_A)^{-1}}{\text{var } (\hat{\gamma}_A)^{-1} + \text{var } (\hat{\gamma}_B)^{-1}}\right)^2 + \text{var } (\gamma_B)\left(\dfrac{\text{var } (\hat{\gamma}_B)^{-1}}{(\hat{\gamma}_A)^{-1} + \text{var } (\hat{\gamma}_B)^{-1}}\right)^2}{\left(\dfrac{\text{var } (\hat{\gamma}_A)^{-1}}{\text{var } (\hat{\gamma}_A)^{-1} + \text{var } (\hat{\gamma}_B)^{-1}}\right)^2 + \left(\dfrac{\text{var } (\hat{\gamma}_B)^{-1}}{\text{var } (\hat{\gamma}_A)^{-1} + \text{var } (\hat{\gamma}_B)^{-1}}\right)^2}$$

$$= \frac{.057\left(\dfrac{\frac{1}{.057}}{\frac{1}{.057} + \frac{1}{.295}}\right)^2 + .295\left(\dfrac{\frac{1}{.295}}{\frac{1}{.057} + \frac{1}{.295}}\right)^2}{\left(\dfrac{\frac{1}{.057}}{\frac{1}{.057} + \frac{1}{.295}}\right)^2 + \left(\dfrac{\frac{1}{.295}}{\frac{1}{.057} + \frac{1}{.295}}\right)^2}$$

$$= .066$$

Thus, we estimate γ to be 1.29 with a standard error of $\sqrt{.066} = .256$ and conclude, with 99 percent confidence, that $\gamma > 0$.

Appendix 2.3

ESTIMATING DURATION DEPENDENCE

Recall Chamberlain's model:

$$\text{Prob}\ (y_{it} = 1 \mid y_{i,t-1}, y_{i,t-2}, \ldots) = \frac{\exp\ (A_i + \gamma_2\, y_{i,t-2})}{1 + \exp\ (A_i + \gamma_2\, y_{i,t-2})}$$

where

$$A_i = \alpha_i + \sum_{k=1}^{\infty} \Psi_{ik} \prod_{j=1}^{k} y_{i,t-j} \ .$$

The procedure for estimating γ_2 is quite similar to that used for estimating γ, described in Appendix 1. We eliminate the individual-specific parameters (A_i) from the equation using a conditional likelihood function, and this reduces our unknowns to only one--γ_2.

Chamberlain has shown that sufficient statistics for α_i and the Ψ_{ik} are S_{i01}, S_{i011}, ..., where, for example, S_{i011} is the number of times in the sequence that 1 is preceded by 01. Also, we must hold the following variables fixed: y_{i1}, $y_{i,T-1}$, n_{iT}, and n_{i1}, where n_{i1} is the number of consecutive 1's at the beginning of the sequence and n_{iT} is the number of consecutive 1's at the end of the sequence. This gives us the conditional log-likelihood function:

$$L = \sum_{i=1}^{N} \ln \left[\exp\ (\gamma_2 \sum_{t=n_{i1}+2}^{T} y_{it}\, y_{i,t-2}) \Big/ \sum_{\underset{\sim}{d}\varepsilon b_i} \exp\ (\gamma_2 \sum_{t=n_{d1}+2} d_t\, d_{t-2})\right].$$

where: $B_i = \{\underset{\sim}{d} = (d_1, \ldots, d_T) \mid d_t = 0 \text{ or } 1,\ n_{d1} = n_{i1},\ n_{dT} = n_{iT},\ d_2 = y_{i2},$

$d_{T-1} = y_{i,T-1},\ s_{d01} = s_{i01},\ s_{d011} = s_{i011}, \ldots\}.$

We need $T \geq 6$ in order to generate any conditional probabilities that depend on γ_2.

Take the case where T = 6, a situation with 64 possible sequences. Here, one set of restrictions produces conditional probabilities that depend on γ_2. This one set of restrictions yields two sequences--(101000) and (100100). Conditional probabilities for these sequences are:

$$\text{Prob } [(1,0,1,0,0,0) \mid (1,0,1,0,0,0) \text{ or } (1,0,0,1,0,0)] = \frac{\exp(\gamma_2)}{1 + \exp(\gamma_2)}$$

$$\text{Prob } [(1,0,0,1,0,0) \mid (1,0,1,0,0,0) \text{ or } (1,0,0,1,0,0)] = \frac{1}{1 + \exp(\gamma_2)}$$

We can obtain an estimate of exp (γ_2) by dividing the number of men with the sequence 101000 by the number of men with the sequence 100100. The natural log of that value gives us an estimate of $\hat{\gamma}_2$.

Chapter 3

RETIREMENT IN PROSPECT AND RETROSPECT

James N. Morgan

INTRODUCTION

In the eleventh wave of interviews of the Panel Study of Income Dynamics, we asked working heads of families between 45 and 64 at what age they planned to retire and what they expected in the way of pension coverage. Retired family heads were asked about their retirement experiences. Younger family heads were not asked any of these questions because their retirement seemed too far in the future. A separate series of questions about first and present jobs was specifically included for those respondents under age 45 (see Chapter 1). The Panel Study's ten years of background information on panel members allow us to examine both plans and actual retirement in a dynamic context and to explore the effects of illness and unemployment and of changes in wage rates on the retirement decision.

Some basic problems of selectivity bias should be faced, because the sample for the analysis dealing with prospective retirement excluded panel members who were already retired and those who were unemployed or out of the labor force, perhaps even temporarily. The sample of retired family heads, on the other hand, excluded panel members who were still working and, hence, underrepresented older persons who chose to retire late. Some limited data are presented which combine the two groups and allow analysis of the expected-or-actual retirement age, but these data still exclude workers over 65 and some others who did not call themselves retired, and they exclude the unemployed.

There is also a possible selectivity bias when we use earlier years of panel data, because that requires eliminating from the analysis these respondents who did not remain household heads for at least a few years. For most men over 45 the problem was not very serious, but it tended to underrepresent women who got married or became widows.

Previous studies have shown that economic factors dominate retirement decisions, except where health coerces a decision. More recently, attention has been focused on the details of Social Security regulations, particularly the earnings test and the implicit "Social Security tax" on earnings for those between 65 and 72 years of age. Assuming the actuarial reduction in benefits upon early retirement is fair, there is no penalty for working before 65, just as there is no benefit for retiring earlier.

Other Analyses of Retirement

L. J. Kotlikoff used the 1966 first-wave survey of the National Longitudinal Survey panel of men aged 45-59 to test effects of Social Security both on life cycle accumulation and on age of retirement (Kotlikoff, 1979). The latter is, in theory, supposed to affect the former, since one must save more if one plans to retire earlier. He found no support for the theory that aggregate savings might be reduced due to the introduction of Social Security. More important for the present context, he found that the "Social Security tax" on the earnings of persons age 65 to 71 showed no significant effect either on expected retirement age or on the likelihood of planning to retire before age 65. He used the ratio of Social Security benefits lost (through working) to full-time earnings as an estimate of Social Security's tax on earnings. He adjusted for selection bias resulting from the fact that there is no such earnings penalty before age 62. If the system were actuarially fair in increasing benefits between ages 62 and 65, then there would be no tax on earnings in that period. Kotlikoff tested this notion by using Heckman's method of introducing a term from a logit regression for probability of being subject to the tax. The results did not indicate any "Social Security effect." Indeed, while he made little of it, Kotlikoff's data also showed substantial effects of private and of non-Social Security government pensions in reducing expected retirement ages. Since a vast majority of people with these pensions also have Social Security (but even those who do not are more likely to plan earlier retirement), we cannot be sure that it is the extra expected retirement income that makes the difference, although it seems likely. There is less of an upper limit on private pensions, fewer earnings tests, and perhaps earlier entitlement.

Anthony J. Pellechio used the 1973 exact match file combining the Current Population Survey, Internal Revenue Service tax returns, and Social Security Administration files (Pellechio, 1977). This allowed him to model the Social Security provisions--the kinky "tax on earnings" more precisely--for a sample of 62 to 70 year old married men entitled to Social Security but not entitled to railroad retirement, not employed by federal state or local government, and not receiving any welfare, unemployment compensation, or disability payments. He defined retirement as having less than $1,680 in 1972 earnings reported in Social Security records, which was the exempted amount at that time. The Social Security data allow precise calculation of individual benefits (or potential benefits for those still working), estimation of the present value of future benefit payments for both husband and wife and even the present value of the increase in future benefits by working another year (assuming some potential earnings). Separate retirement models were estimated for those age 62 to 64 and those 65 or older. Pellechio found that the level of expected Social Security benefits had a significant positive effect on retiring, but he found no such effect for the expected increase in benefits from working one more year. (This finding agrees with a model which disregards substitution effects and shadow prices for leisure and assumes that people have income goals which dominate their work and retirement decisions.) He also found an effect on retirement of the amount of work required just to earn the amount exempted in the earnings test. But earnings level was also used directly and had a negative effect on retiring. And it was used again to estimate the pension benefit from working another year, so a great deal hangs on three aspects of the same variable. However, the ability to estimate the effect of earnings on the current value of expected social security benefits allowed Pellechio to separate that effect from any other effect of earnings level, leaving the latter to have a negative effect on the probability of retiring. Earnings enters in a third way as affecting the hours of work necessary to earn the exempt amount, and a significant coefficient is interpreted to mean that raising the exempt amount might discourage retirement (encourage more work).

A significant effect of a wife's wage in postponing her husband's retirement is interpreted as a lowering of the husband's "shadow price" of time somehow, rather than as encouraging the wife to keep working and the husband to continue working also so they can retire together. More complex models of retirement include a relationship in which expected age of retirement is affected by expected retirement income, but also affects retirement income by affecting current saving. A recent example was an estimate of the Social Security wealth of individuals which used the 1965 Federal Reserve Survey of Financial Characteristics of Consumers and found that Social Security reduces private saving (Feldstein and Pellechio, 1979). There is a wide disparity in these estimates, however, depending on whether the data are weighted by the inverse of some widely differential sampling fractions, which means that the models are probably misspecified (DuMouchel and Duncan, forthcoming).

An article by Joseph F. Quinn used the 1969 wave of the Retirement History Survey to analyze who in that sample, 58 to 61 years of age in 1967, were still working in 1969. Quinn found that poor health, pension coverage, and asset income encouraged retirement, but higher wage levels did not. However, not having attempted to estimate "shadow wages" for those already retired, he was confronted by a missing variable problem in estimating wage effects. In another paper, Quinn reported that the self-employed retire for much the same reasons as the employed (Quinn, 1979).

Dena K. Motley, using the Retirement history Survey, showed that most of the (recently) retired in that panel were unable or unwilling to return to work. Karen Schwab reported on early retirement from the same study. Virginia Reno looked at reasons people stop working as indicated by data from the Survey of New Beneficiaries. Richard V. Burkhauser used the Panel Study of Income Dynamics data to estimate present expected value of pensions and present value of future income if one does not retire, using the difference to account for the decision to retire.

Two earlier studies focusing on labor force participation used respectively, a micro sample from the 1960 Census and some data from the Current Population Survey. (Bowen and Finegan; Cohen, Rea, and Lerman, 1970) Both used unearned income to help explain labor force participation, but that included Social Security and pensions which are a result rather than a cause of retirement. In addition to the data sources used above, there was a study of pension coverage done in 1972. It was repeated in the spring of 1979 (U.S. H.E.W.).

A number of studies by Herbert Parnes and colleagues based on the panel of older men in the Labor Department - Ohio State University National Longitudinal Study appeared in five volumes, the first four entitled The Pre-Retirement Years and the fifth called From the Middle to the Later Years: Longitudinal Studies of the Pre- and Post-Retirement Experiences of Men (1979). A summary appears in a paper given by Parnes at the Conference on Demographic and Health Information for Aging Research, Bethesda, Maryland, June 1979.

Among other things, the NLS data indicate clearly that people want to retire, and that compulsory retirement ages are relatively unimportant. A categorization of retirees according to the "route to retirement" based on information both before and after retirement seemed more reliable than retrospective information alone and was used to describe the differences in the various groups according to their reasons for retiring. The importance of poor health in driving people into retirement, in producing and affecting their economic and attitudinal futures, was emphasized. And the finding that most retired people are happy with their situation agrees also with our data, as we discuss below. Both confirm an earlier study by Streib and Schneider (1971).

Previous work by the Institute for Social Research has been reported by Barfield and Morgan (1968, 1978). A national sample in 1966 was compared with smaller national samples in 1968 and 1963 and with a sample of United Auto Workers Union members aged 58 to 61 who were interviewed in the summer of 1967. Economic factors and health proved to be the dominant factors in retirement plans and also in the satisfaction experienced by retirees. There appeared to be a tendency for younger people to be more likely to plan early retirement, and there was a trend over time toward earlier retirement, at least in plans. This raised the question whether there was an age effect, with people becoming more realistic about the possibility of early retirement as they got older--or whether this represented a secular trend, with each new generation less imbued with the work ethic. A reassessment based on a small sample in 1976 showed that neither was true, that there were perhaps some age cohorts coming along with unusually high expectations about early retirement, but that some still younger generations, presumably with less favorable employment experiences, were less likely to expect early retirement.

Marjorie Honig and Giora Hanoch are working on a complex analysis of the labor market behavior of older people using three-wave merged data from the Retirement History Study, one sample for men and one for women household heads (Honig and Hanoch, 1979). They are proposing to fit a complex six-equation model combined with two sets of wave-to-wave changes to allow for time trends or cohort effects and are working in stages to allow optimum specification searches. They propose to use the Heckman procedure also to deal with selection biases. (Logit estimation of probability of participation and of being on each of the segments affected by different Social Security provisions provides the inverse Mill's ratios which are then used as additional explanatory variables in the other equations to eliminate selectivity biases.)

By matching the Social Security Administration's Longitudinal Employer-Employee Data file (LEED) and the U.S. Department of Labor's file of private pension plan details, Bradley R. Schiller and Donald C. Snyder showed that changes in pension and retirement provisions did have effects on the quit-rates of different age groups of workers (Schiller and Snyder, 1979).

A national study of attitudes of employees, retirees and business leaders toward retirement was recently published as part of the hearings before the Select Committee on Aging of the House of Representatives (U.S. House, Select Committee on Aging, 1979).

Marian Sobol used the older male worker's panel of the Labor Department - Ohio State National Longitudinal Study to study asset accumulation on the eve of retirement (Sobol, 1979). She found that assets (liquid plus investment, but excluding pensions and insurance) were highly related to income. And they were negatively related to private pension coverage, significantly so only for whites.

A MODEL OF RETIREMENT

The relationships among the panel data available can be modelled in the manner shown in Figure 3.1. While we shall not attempt to estimate such a full model, it is useful as background. There was a natural breaking point in 1972, the last year when some attitudinal questions were asked, so we can think of the 1967-1972 experience of illness, unemployment, disability, and change in earnings as affecting achievement orientation, sense of efficacy, and reported enjoyment of one's job in 1972. Of course, basic background factors like age, education, sex, race, and occupation affect everything. Age, as we have seen, reflects both chronological age and the period of history when the individual grew up and entered the labor force. Only repeated measurements of retirement plans and explanations of actual retirements can untangle the influences.

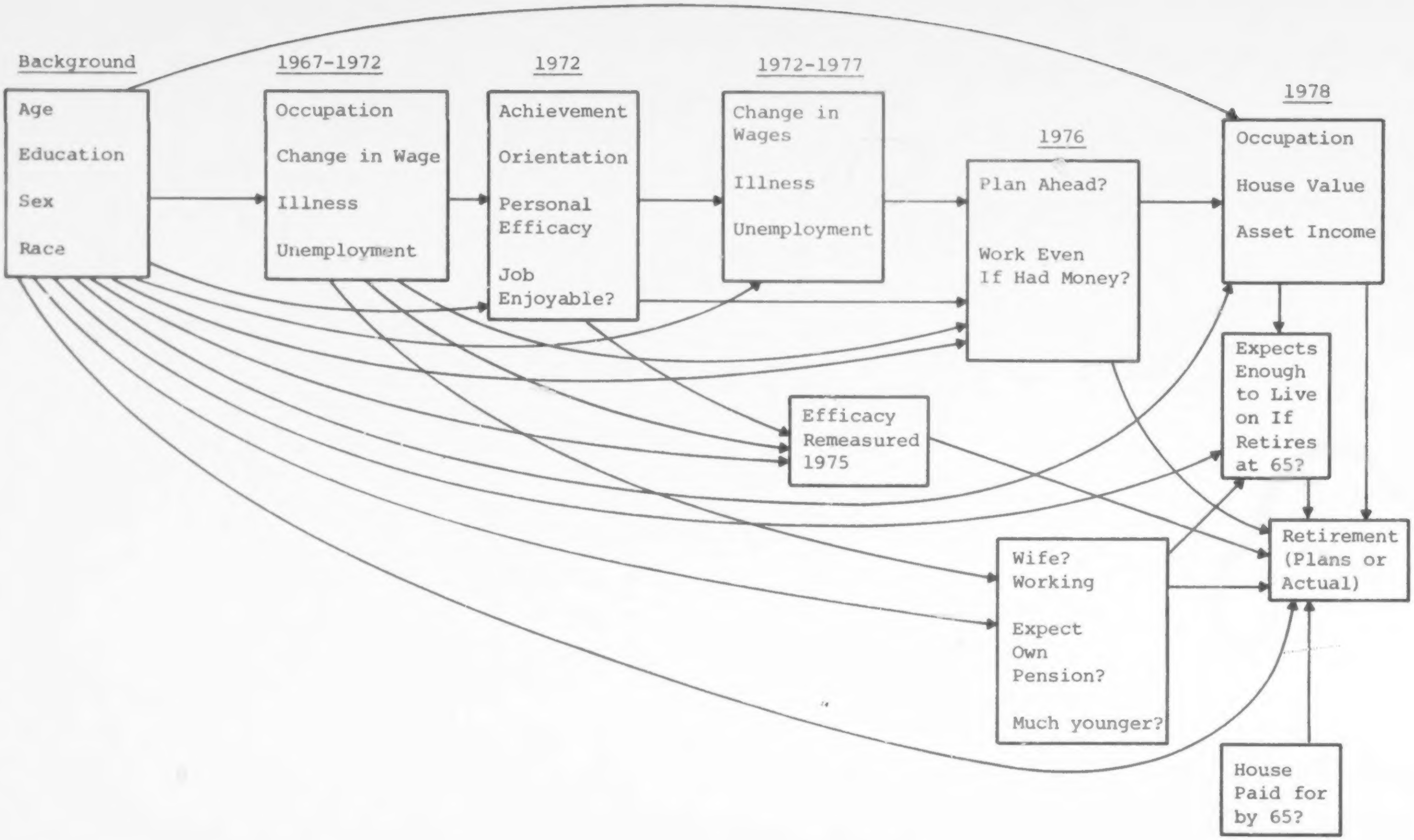
All this background experience and attitudes through 1972 can be expected to influence in turn the experience between 1972 and 1977 of increased earnings, and of illness and unemployment. But we also have intermediate measures of personal efficacy in 1976, reported tendency to plan ahead in 1976, and whether the respondent thought in 1976 he or she would continue working even if there were plenty of money without it.

All this brings the respondent to 1978, along with a wife who may be working, may expect pensions of her own, and may be younger than the respondent. The respondent is asked for a summary of the economic situation, that is, whether he or she would have enough to live on if he/she retired at 65. The final result of all this is retirement, planned or actual, though for past retirements we have less information. This chapter represents some first steps investigating parts of the model.

The sub-models we work with in this chapter are much simpler, and we do not even attempt to separate the effects of such exogeneous background variables as

FIGURE 3.1

A MODEL OF RETIREMENT


Background
Age
Education
Sex
Race
1967-1972
Occupation
Change in Wage
Illness
Unemployment
1972
Achievement
Orientation
Personal Efficacy
Job Enjoyable?
1972-1977
Change in Wages
Illness
Unemployment
1976
Plan Ahead?
Work Even If Had Money?
1978
Occupation
House Value
Asset Income
Efficacy Remeasured 1975
Expects Enough to Live on If Retires at 65?
Retirement (Plans or Actual)
Wife? Working
Expect Own Pension?
Much younger?
House Paid for by 65?

education, sex, race, and age directly from their effects through earnings, expected pensions, health, or whether the house will be paid for. In justification we may say that the background variables for the most part do not seem to matter much anyway.

Is There Abrupt Retirement, or Is It Only a Gradual Process?

Can we base an analysis on replies to questions about what year the respondent retired, or planned to retire? Or is there a vague process of retiring from or losing one job and gradually working less and less? By pooling all those in the panel who retired in 1969 through 1975, we can look at their work hours for the five years surrounding their reported retirement year. Table 3.1 shows that there was indeed a precipitous drop, except for those who retired quite late, from full time or nearly full time work to only a few hundred hours per year on the average. The implication is that there is indeed "retirement" and that it is not gradual for most people. The few who retired very late, however, had obviously cut their hours previously.

An abrupt reduction in hours may or may not mean an abrupt decline in economic status, even if we ignore non-money income such as a rent-free owned home. Table 3.2 shows that for most respondents there was a drop in income/needs decile position of several deciles, whenever they retired.

Consistency between Current Reports on Work Status and Later Report on Year of Retirement

We have some evidence on the accuracy of reports on the age of retirement. In 1978 the retired respondents in the Panel Study of Income Dynamics were asked "In what year did you retire?"

In previous interviews, conducted from 1967 through 1977, they were asked each year, "We would like to know what you do--are you (head) working now, looking for work, retired, a student, (a housewife), or what?"

In the last two of these years a separation was made in coding between those only temporarily laid off and those looking for work and unemployed. And among those not in the labor force, the retired were distinguished from the permanently disabled, housewives, students, and others. Hence, there is some natural ambiguity, since a person could have reported being neither in the labor force nor retired when he or she was a young student or an older housewife or permanently disabled.

Table 3.1

ANNUAL WORK HOURS FOR THE FIVE YEARS CENTERED ON THE YEAR OF RETIREMENT
(For 274 constant family heads since 1968 who retired in 1969-1975.)

	Age of Retirement					
Work Hours	Before 62	62-64	65	66-71	72 or later	All
2 years before retirement	2030	1851	2097	1467	813*	1799
1 year before retirement	1863	1702	1813	1252	909*	1610
year retired	922	1020	620	703	664*	842
1 year later retirement	323	265	181	259	319*	269
2 years later retirement	223	181	142	206	369*	205
Percent of subsample	25	32	16	20	8	
Mean change in income/needs decile position from 2 years before to 2 years after	-1.82	-1.77	-2.62	-0.56	-0.68*	-1.60

*Fewer than 25 cases (n=16).

Table 3.3 shows the proportions of respondents who reported being in the labor force (working, temporarily laid off, unemployed) in each year according to year of retirement (as reported in 1978). The bottom part of the same table reports the proportion who said in each year that they were retired.

Overall, there was substantial consistency. Only a small group said they were working for a year or two after the year they thought of as their year of retirement, but there was not enough discrepancy to justify fitting a curve to annual hours of work and finding the inflection point to define "retirement" more precisely.

There also seemed to be somewhat more discrepancy when there was a greater elapsed time since retirement, but these differences are not great.

RETROSPECTIVE REPORTS ON RETIREMENT

When asked why they retired when they did, four-fifths of the respondents mentioned that they were eligible, or were tired and wanted to retire, or that their health dictated it. A few mentioned family reasons, including the health of other family members, and still fewer mentioned financial considerations. Only a few gave second reasons. Table 3.4 takes the three largest groups

Table 3.2

CHANGE IN FAMILY INCOME/NEEDS DECILE FROM TWO
YEARS BEFORE RETIREMENT TO TWO YEARS AFTER
(For 274 constant family heads since 1968 who retired 1969-1975)

Change in Income/Needs Decile	Age of Retirement					
	Before 62	62-64	65	66-71	72 or later	All
Down 60 or more	8%	9%	11%	2%	0%*	7%
Down 3-5	24	31	46	22	30*	30
Down 1-2	40	34	22	27	28*	32
No change	16	10	4	30	12*	15
Up 1-2	5	8	5	7	13*	7
Up 3-5	4	4	12	4	17*	6
Up 6 or more	3	4	0	7	0*	4
	100	100	100	100	100	100
Percent of retired subsample	25	32	16	20	8	

*Based on fewer than 25 cases.

according to the first reason they gave for retiring when they did and looks at what else they said about their retirement. Those who retired because of ill health were more likely to have retired unexpectedly and to have been unwilling or sorry to retire. They were less likely to have worked since retirement, though no less willing to say they would have worked if a job were available. They were much more likely to say they were worse off than before retirement than to say they were better off.

The main difference between those who gave age or eligibility as a reason and those who said they just wanted to retire is that the latter were less likely to have retired as they had planned and expected and less likely to have worked after retirement, but they were more likely to say that, considering income and expenses, they were living as well as or better than before retirement. People's general feeling about life since retirement showed the same range from predominantly good or very good for those who retired when they were eligible, mostly as planned and willingly, to a more nearly even distribution of good and bad for those whose health drove them to retirement, often unexpectedly and unwillingly.

Table 3.3

REPORTS BY RESPONDENT THAT HE/SHE IS CURRENTLY WORKING BY YEAR OF INTERVIEW AND YEAR RESPONDENT REPORTED RETIRING IN 1978 INTERVIEW
(For 422 retired constant family heads since 1968.)
(Percentage)

Year Reported Retired (in 1978 Interview)	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978*
Currently Says is Working or Unemployed											
1967	27	26	17	10	10	10	10	0	0	10	0
1968	59	10	6	11	7	10	4	7	7	0	0
1969	76	60	14	5	14	9	5	5	4	4	0
1970	77	73	62	19	14	7	0	0	0	3	0
1971***	99	95	96	78	16	15	0	7	7	9	0
1972	98	94	97	93	47	17	6	3	0	0	0
1973	93	89	87	89	81	58	22	5	0	0	0
1974	96	96	93	93	85	81	44	17	8	5	0
1975	90	94	84	88	92	88	84	59	12	3	0
1976	95	91	91	94	91	90	91	83	65	1	0
1977	89	97	97	97	91	97	94	93	90	71	0
1978	100	100	100	97	97	100	97	97	97	100	100
Currently Says is Retired											
1967	66	71	82	90	90	90	90	100	95	79	100**
1968	37	86	92	74	92	90	88	91	86	95	100
1969	14	40	81	85	76	90	95	95	91	91	100
1970	16	20	38	70	80	90	99	100	96	96	100
1971***	1	4	4	18	80	77	95	93	87	81	100
1972	6	6	3	7	53	83	94	94	93	98	100
1973	7	9	10	9	13	40	75	90	100	99	100
1974	0	4	7	8	15	19	50	82	83	86	100
1975	5	8	15	12	8	4	16	40	82	92	100
1976	5	8	8	3	8	8	8	13	30	96	100
1977	8	3	3	0	9	3	6	7	10	25	100
1978	0	0	0	3	0	0	3	3	0	0	100

*Not asked the question sequence

**Automatic consistency.

***Table 3.3 reads, using the rows labeled 1971: Of those who reported in 1978 that they retired in 1971, 99 percent reported in 1968 that they were working or looking for work, 95 percent in 1969, 96 percent in 1970, 78 percent in 1971, 16 percent in 1972, etc. And the same group in the lower part shows that the percent reporting they were retired was 1 percent in 1968, and in succeeding years, 4 percent, 4 percent, 18 percent, 80 percent, etc.

Table 3.4

CORRELATES OF REASONS FOR RETIRING
(For 479 family heads who retired in 1967-1978.)
(Percentage of subgroup.)

Descriptive Variables	Of Those Who Retired When They Did Because of: Age Eligibility	Health	Tired, Wanted to	All
Retired unexpectedly	14	82	20	40
Had planned to retire then	80	15	71	52
Retired or unemployed because of ill health	4	74	9	23
Were willing or even glad to retire	69	24	77	53
Worked since retirement	35	22	27	31
Worked in 1977	37	23	29	33
Does volunteer work	38	24	37	32
Would work if a job were available	33	32	33	32
Mentions an explicit job would take	18	20	16	19
Considering Income and Expenses, is:				
Living as well as/or better than before retirement	69	54	82	65
Living not quite as well	19	32	11	23
Living much worse	5	12	2	6
Feels has enough to live comfortably	88	70	86	79
Generally Speaking, Feels About Life Since Retirement				
Very good, enthusiastic	29	18	25	23
Good	39	24	54	39
General, pro/con	18	27	15	18
Bad, not good	6	21	2	9
Very bad	2	5	0	3

We now look at how the retired felt about their life since retirement and what they said about the experience. Table 3.5 shows even more dramatically the correlations of negative feelings about retirement with retiring unexpectedly, for health reasons, or because one had to. Working for money, or wanting to, seems to have little to do with current satisfaction, but doing volunteer work apparently does make people feel better about their lives since retirement. The causation could go either way, but many gerontologists think that such involvement and activity does, in fact, make people feel better. There was little difference in the good feelings of those who said they would like to work if a job were available and those who said they would not.[1]

We can also see how groups which differed in how they felt about retirement actually fared in terms of their income or changes in income. Sliding individual scales as before to combine data according to its distance from the retirement year, we look at the change from two years before to two years after retirement. Table 3.6 shows that there was very little pattern to the reduction in work hours between those who were satisfied or dissatisfied with retirement. Even the change in income/needs decile position was not monotonically related to satisfaction with retirement. What the satisfaction questions correlated with most strongly was the current income/needs position of the family. Those in a low decile were more likely to report that they were worse off than before they retired, but they were also worse off than others before they retired.

We should not make too much of the absolute level of satisfaction reported by the retired. It has long been known that the older people are, the less likely they are to complain about anything. This exemplifies the problem of age, period, and cohort effects. We do not know whether people simply get used to things and learn to live with what they have, or whether the older generations have a different philosophy of life, or whether older people are more aware of the long upward trend in living standards.

ARE PLANS REALISTIC?

It is evident from other studies that many fewer people plan to retire for health reasons than actually do. Some analysts think that people may claim to have retired because of ill health when in fact they did not, but it seems to

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[1] These means are based on very few cases and hide a substantial variability, hence should not lead us into too much theorizing. Finally, of course, those who are satisfied with their lives also feel that they live as well as before retirement and have enough money to live on comfortably.

Table 3.5

CORRELATES OF FEELINGS ABOUT LIFE SINCE RETIREMENT
(Percentage of subgroup.)

Descriptive Variables	Feelings About Their Life Since Retirement				All*
	Very Good	Good	Neutral	Bad or Very Bad	
Retired unexpectedly	30	34	49	64	39
Retired for health reasons	14	17	33	43	23
Retired because had to	25	27	56	70	37
Worked since retirement	27	38	24	35	31
Worked in 1977	30	41	22	29	33
Does volunteer work	50	33	22	14	32
Mentions an explicit job might take if available	25	18	15	23	19
Considering income and expenses, is-					
Living as well as or better than before retirement	81	77	53	42	65
Living not quite as well	15	18	38	34	23
Living much worse	3	3	7	23	6
Feels has enough to live on comfortably	97	89	75	53	79

*Including some who expressed no evaluation.

them to be a more acceptable reason for retiring than some others. It seems at least as plausible that deteriorating health really changes people's retirement decisions.

Even though there might be a trend toward earlier retirement, because of cohort or period-of-history effects, it is useful to compare the ages at which retired people reported having retired with the ages at which those who were still working said they would retire. Table 3.7 shows that people generally

Table 3.6

CHANGES FROM TWO YEARS BEFORE RETIREMENT TO TWO YEARS AFTER
(For those who retired in 1967-1975.)

Descriptive Variables	Income/Needs Decile			Family Money Income			Hours Change	Income/Needs Decile in 1977	N
	2 years Prior	2 Years After	Change	2 Years Prior	2 Years After	Change			
All	5.16	3.58	-1.58	$10,493	8,656	-1,837	-1,517	3.64	314
"Considering income and expenses, are you living better than before you retired about as well, not quite as well, much worse or what?"									
Better	5.00	3.99	-1.01	10,726	11,080	334	-1211	4.40	34
About as well	5.47	3.93	-1.54	10,721	9,116	-1605	-1631	3.95	161
Not quite as well	4.94	2.75	-2.19	10,532	7,323	-3209	-1496	296	87
Much worse	3.25	2.90	-0.35	8,081	7,623	-458	-1202	2.46	23
"Do you feel you have enough to live on comfortably?"									
Yes	5.26	3.83	-1.43	10,830	9,167	-1663	-1534	3.93	232
No	4.19	1.95	-2.24	8,205	6,095	-2110	-1438	1.86	71
"Generally speaking how do you feel about your life since retirement?"									
Very good	6.08	4.35	-1.73	12,270	9,580	-2690	-1721	4.46	67
Good	4.92	3.68	-1.46	10,101	9,311	-790	-1383	3.55	122
Pro-con	4.45	2.53	-1.92	9,931	7,742	-2189	-1626	2.93	68
Bad	3.97	3.06	-0.91	8,432	7,639	-793	-1319	3.23	31
Very bad	5.98	3.30	-2.68	13,613	8,411	-5202	-1634	3.00	11

retired earlier or later than plans of workers would indicate. One would conclude that some individuals who say they will retire after 71 or never, will retire before then, and that some workers who say they will retire at 65 will actually retire earlier or later.

Even though we followed up by asking those who said they did not know just when they would retire whether they would retire before 65 it is still true that a single estimated age of retirement is not an ideal dependent variable. In our earlier study on retirement we showed that a composite variable made up of a number of indicators of interest in early retirement, had a much stronger and more nearly linear relationship with such economic variables as expected retirement income (Barfield and Morgan, 1969).

AGE, PERIOD AND COHORT EFFECTS

The first studies of retirement plans in 1966 showed a dramatic difference among age groups in cross sections, young people being more likely to say they planned to retire early. That could of course reflect either an age effect of older people incurring obligations or holding rewarding jobs or having other reasons for deferring retirement, or a cohort effect where each succeeding generation is less imbued with the work-ethic and more interested in retiring to enjoy life and is perhaps covered by better pension plans. ISR's 1976 reassessment led to the impression that there were cohort effects, because a new younger cohort appeared less likely to report plans to retire early than an older cohort (Barfield and Morgan, 1978). We surmised that perhaps the older cohort had had a much more favorable employment and earnings experience, having entered the labor market in good times and secured seniority and good jobs before unemployment became a serious problem. With increasing inflation, however, there may well be a "period of history" effect, with retirement plans depressed because of the fears and uncertainties and real capital losses caused by inflation. Table 3.8 shows the proportions of different age groups and cohorts planning to retire before age 65--an excellent cut-off point that divides the population and maximizes our capacity to discriminate. It is clear that the post-war generation kept and even increased its commitment to early retirement, while a still younger generation lost its enthusiasm for early retirement over the years.[2]

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[2] For a study of cohort effects on earnings and a discussion of possible permanence of cohort effects, see Freeman (1979).

Table 3.7

ACTUAL OR PLANNED RETIREMENT FOR RETIRED HEADS AND WORKING HEADS 45 OR OLDER
(Percentage)

Age in 1978	Retirement Before Age 65: Plans of Working Heads	Retirement Before Age 65: Actual Retirement Age of Retired	Retirement Before Age 65: All	Retirement Before Age 62: Plans of Working Heads	Retirement Before Age 62: Actual Retirement Age of Retired	Retirement Before Age 62: All
Less than 50	47	100*	47	25	100*	26
50-54	51	100*	53	25	100*	27
55-59	49	100*	53	16	100*	22
60-64	29	100*	53	2	79	28
65-69	**	80	-	**	29	-
70-74	**	50	-	**	18	-
75 or more	**	35	-	**	11	-
Number of cases	1202	624	1826	1202	624	1826

Retirement Age	Plans of Working Heads Age 45-64	1978 Plans of Those Who Have Been Heads Since 1972	Actual Retirement Age for All Retired Heads	Actual Retirement Age for Heads Who Retired 1967-1978
Under age 62	20%	20%	29%	25%
62-64	27	22	32	32
65	38	33	12	16
66-71	4	4	18	20
72 or older	12	22	8	8
Total	101	101	99	101
Number of cases	1202	1066	614	479

Note selection bias: non-retired over 65 are excluded from both groups, as are unemployed.
* Fewer than 25 cases and automatically 100 percent.
**Not asked.

Table 3.8

RETIREMENT PLANS BY AGE AND AGE COHORT
(Percentage planning retirement before age 65.)

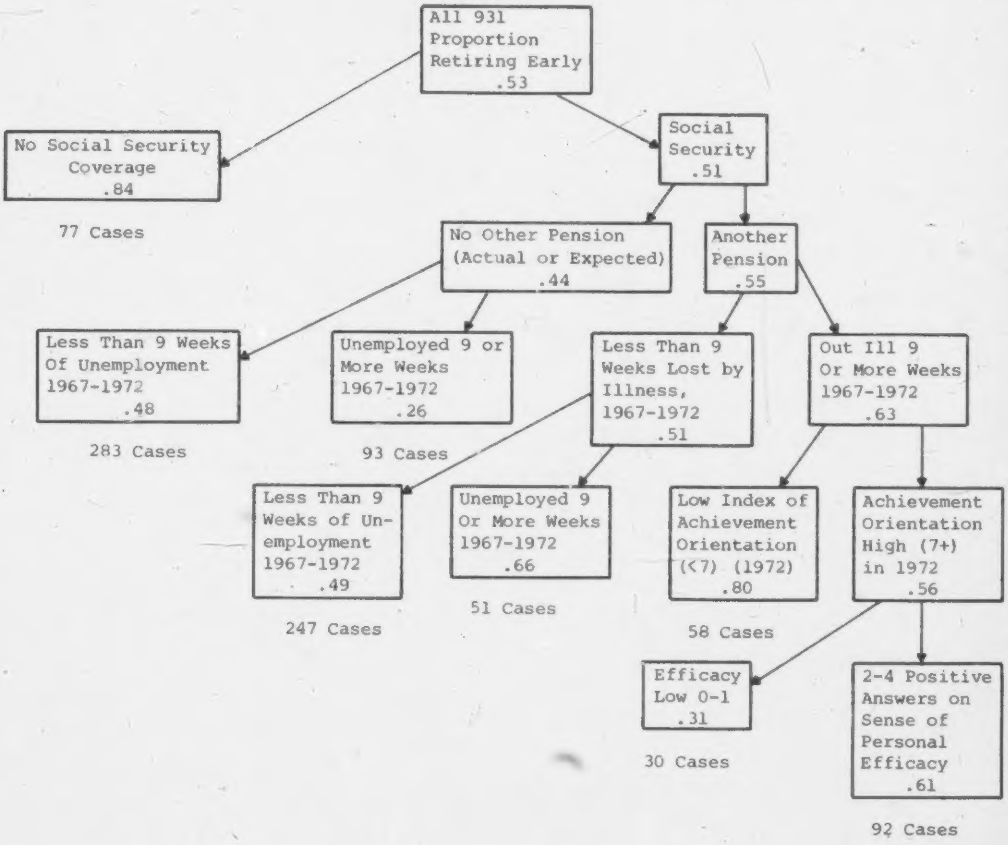
Age	1966	1976	1978
Age Groups (Age at Interview)			
35-39	42	55	-
40-44	38	40	-
45-49	34	36	47
50-54	30	46	51
55-59	25	42	49
60-64	11	17	29
Age Cohorts (Age in 1966)			
35-44	42	36	51
40-44	38	46	50
45-49	34	53	56
50-54	30	17	21

WHO RETIRES EARLY, OR PLANS TO?

Even though there is some tendency for people to plan to retire at 65 but in fact retire earlier or later, the selectivity bias from dealing either with plans (omitting those who have already retired) or actuality (omitting those who are still working) compels at least a brief look at the combined groups. The results of our search for what factors affected actual or expected retirement age are shown in Figure 3.2. Pension coverage was clearly dominant, followed for those with only Social Security by a discouraging effect of unemployment on retiring early--presumably because of a desire to make up for the losses, and an effect of illness encouraging early retirement for those with two pensions. People with two pensions were also more likely to retire early if they had suffered unemployment in 1967-1972. (We used that early period for both illness and unemployment in order to place the experience ahead of the decision for most of the sample, even those already retired.)

Among the group with the most capacity and incentive to retire early—those with two pensions and some illness experience--attitudes seemed to matter; low achievement orientation or a high sense of personal efficacy encouraged early retirement.

Figure 3.2
WHETHER DID OR WILL RETIRE BEFORE AGE 65
(For 931 household heads working or retired in 1978 who had been heads since 1968.)


All 931 Proportion Retiring Early .53
No Social Security Coverage .84
77 Cases
Social Security .51
No Other Pension (Actual or Expected) .44
Another Pension .55
Less Than 9 Weeks Of Unemployment 1967-1972 .48
283 Cases
Unemployed 9 or More Weeks 1967-1972 .26
93 Cases
Less Than 9 Weeks Lost by Illness, 1967-1972 .51
Out Ill 9 Or More Weeks 1967-1972 .63
Less Than 9 Weeks of Unemployment 1967-1972 .49
247 Cases
Unemployed 9 Or More Weeks 1967-1972 .66
51 Cases
Low Index of Achievement Orientation (<7) (1972) .80
58 Cases
Achievement Orientation High (7+) in 1972 .56
Efficacy Low 0-1 .31
30 Cases
2-4 Positive Answers on Sense of Personal Efficacy .61
92 Cases

We are of course limited in our use of explanatory variables when we include people already retired, and we must infer from present pension income that they knew what pensions to expect. Furthermore, even if we include in our analysis only workers who were aged 45 or older and were asked the questions, we have a reasonably large sample still planning retirement that deserves attention. We turn now to them.

Retirement Plans as Conditional Decisions

If we think of a person first deciding whether he wants to retire before he is age 62, or if not that whether he plans to retire between 62 and 65, or if not that at 65 rather than later, we have three analyses of percentages which can be handled in the same way as probabilities of death. That is, multiplying the inverse probabilities (the probabilities of not retiring), will give the proportions still working at 65 and at 66. For example, of the 1066 respondents who were constant household heads since 1972, and 45-64 years old, 20 percent planned to retire before age 62. Of the remaining 882, 32.7 percent planned to retire before age 65, and of the remaining 663, 74.1 percent planned to retire at 65 rather than later. This means that (1 - .20) (1 - .327) = .538 would be the expected proportion still working at 65, and .538 (1 - .741) = .139 would be the expected proportion still working at age 66.[3]

A number of other analysts have argued that decisions to retire very early, early, or late need to be studied separately since they may be affected by different forces. The conditional decision mode is a straightforward, easily interpreted way to do this, in comparison with multivariate logit which deals with the log odds of retiring in some interval rather than some other interval, usually after 65 or "never."

Unfortunately, preserving the same basic sample so that the multiplications will all work out conflicts with the need in analyzing plans to retire before 62 to exclude those who are already 62-64 years old, but we do not want to exclude them from the analysis of those who plan to retire before 65 or at 65 rather than later. We have compromised, and our three parallel analyses are based on those:

1) planning to retire before 62: 979 family heads currently working and unchanged as heads since 1972 (so we can use information from those earlier interviews), 45-61 years old.

2) planning to retire before 65 if not before 62: 882 heads currently working and unchanged as heads since 1972, 45-64 years old, and not planning to retire before 62.

3) planning to retire at 65 rather than later: 663 family heads currently working and unchanged as heads since 1972, 45-64 years old, and not planning to retire before 65.

Our explanatory model is an attempt to untangle the various economic forces affecting the retirement decision, as well as the constraints or other incentives such as poor health. We do not attempt to model the details of the Social Security earnings test since our focus here is on plans (and it is an era when those rules are likely to change) and since there is only a real tax on earnings between ages 65 and 72 (assuming the actuarial reduction for retiring before 65 is fair), and there is no earnings test after 72.

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[3] The percentages are weighted to be representative and do not correspond with the number of interviews.

The economic forces are all indicated or measured by the same restricted set of variables, but it is useful to see whether they can be identified and tested. There should be some desired standard of living after retirement, based on previous family income/needs and on expected reductions in some living costs and taxes upon retirement. There is an expected retirement income, resulting from previous earnings history, pension coverage, and other savings (house and liquid assets). And there is an opportunity cost of retiring reflected in current hourly earnings, a kind of shadow price of leisure, though other things also affect that.

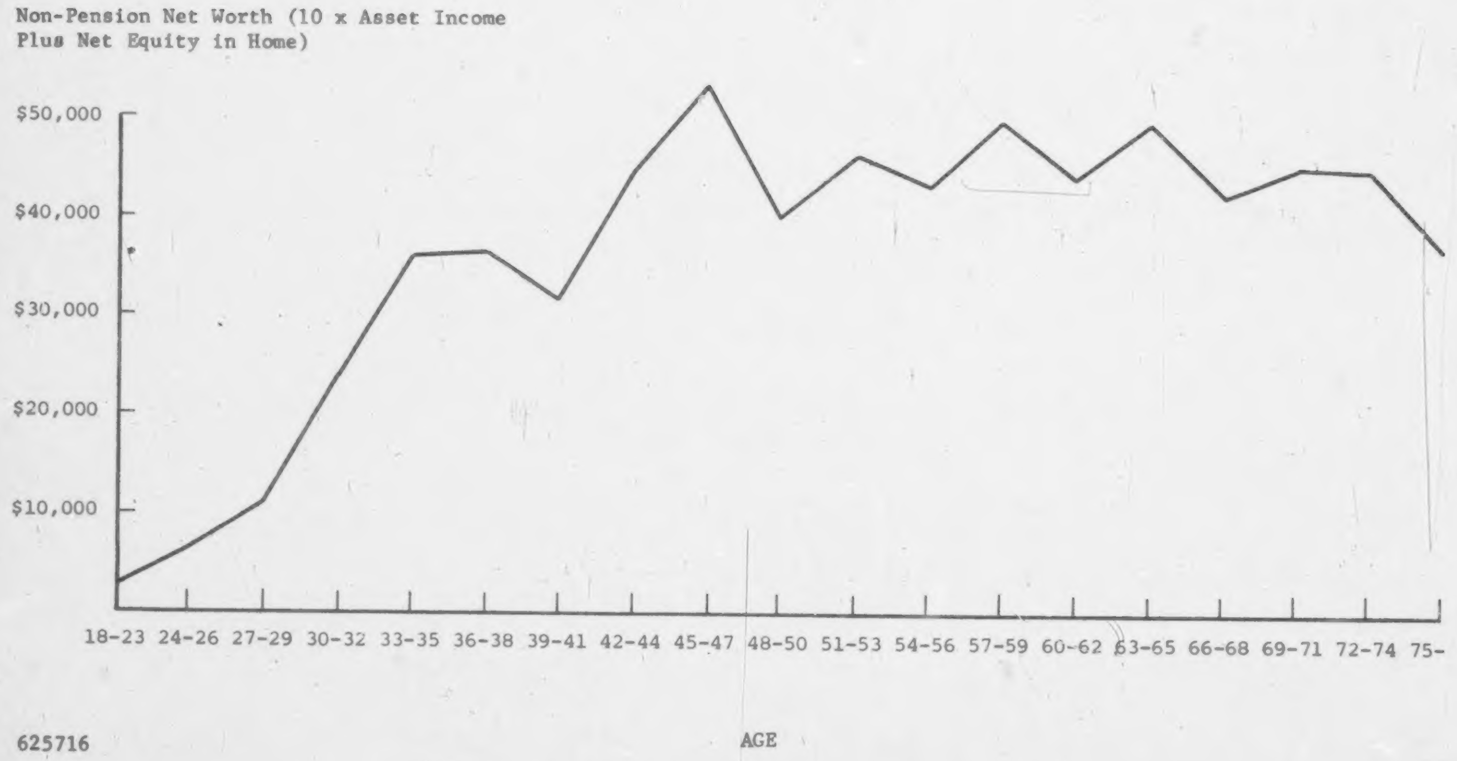
In fact, there are other indicators, such as whether the family head has a working wife and whether she has pension coverage, and a direct question asking whether the respondent thinks there would be adequate retirement income if he/she were to retire at age 65.

Non-pension assets for retirement are indicated by house value and currently reported income from financial assets. Presumably, families accumulate such assets just as they accumulate pension rights, so the current values would increase with age, so we should estimate age-adjusted asset values. When we examined the data, however, even using net equity in the home which should increase as the mortgage is paid off, we found no appreciable differences by age in the average non-pension net worth after the age of 45 (see Figure 3.3). Clearly, if individuals accumulate assets after 45, this is masked by a higher level of assets for each succeeding (younger) cohort, and the whole meaning of normal assets for an age group is suspect. Hence, we used the previous year's asset income and current house value as variables in the analysis. We used house value rather than net equity since only a very small minority expected to owe on mortgages after 65 (a dummy-variable representing this small group is also used).

Even when we exclude those 62 or older from the analysis of plans to retire before 62, the age effect on retirement plans is biased since those who were already retired were excluded. We have already noted the cohort differences in plans to retire, but do not use age cohorts in the analysis here. If the cohort differences are the result of earnings histories and accumulated assets, those variables should take over the explanation.

FIGURE 3.3

ACCUMULATION OF NON-PENSION ASSETS



There remains a problem with some overlapping environmental and personal variables, particularly occupation, working for the government, and private pension coverage. We could assume that those who reported expecting a non-social security pension and who also worked for the government were expecting a government pension. But working for the government was associated with only a few occupations and may have an influence even apart from that because of the nature of the government jobs even within occupations. Table 3.9 illustrates the differences in retirement plans across occupation groups with those with substantial numbers of both government and non-government workers separated. There were not enough non-government teachers to separate, nor enough government workers in the other non-divided occupation groups.

We turn now to the analysis of retirement plans as a set of conditional expectations: of retiring before 62 (for all respondents 45-61 years old), of retiring before 65 (for those 45-64 who do not expect to retire before 62), of retiring at 65 (for those 45-64 who do not expect to retire before 65), and, for that same group, expectations of retiring before 72 (Table 3.10). Note that this modeling has a statistical advantage that the overall proportions are not so close to zero or 100 percent as to make the use of regression with a dichotomous dependent variable unwise. The heteroscedasticity of the dependent variable is not serious, nor the fact that we use an additive model with a limited dependent variable (probabilities cannot be below zero or above 1.0). In such situations, with samples of reasonable size, more elegant techniques such as multivariate logit provide only modest gains in precision (bias is not a problem) at the expense of presenting findings in terms of difficult to interpret log-odds rather than expected probabilities.

It is clear first of all that there are substantial differences in what affects the different decisions, a fact that tends to be hidden if one looks at overall probabilities of retiring before age 62, before 65, and before 66, since the second includes the first and the third includes the first two.

Since negative findings are also important, the reader should keep in mind that the following factors were eliminated from the final regressions because they did not appear to have a significant effect on any of the decisions:

Race

Woman heading a family

Occupations other than manager or operative

Education levels other than high school graduate

Sense of personal efficacy in 1972 and 1975 (which is correlated with economic status and tends to disappear in the multivariate context)

Achievement motivation measured in 1972

Wife's wage rate and its change from 1972 to 1977

Asset income (as a proxy for non-pension wealth)

Head's average labor income 1971-1977 (as a proxy for accumulated pension rights and savings)

Average family income/needs decile (as a proxy for living standards)

The last two in particular were highly correlated with current wage rate, but the latter dominated even when the three were used together.

The striking result, shown in Table 3.10, was that different factors affected the different decisions to retire. Only one factor--belonging to a union that covers one's current job--systematically encouraged retirement at each decision point. Given this disparity, which was expected, it seems best first to discuss the findings for each decision-point separately and then return to patterns or differences.

Plans to Retire Before Age 62

Social Security coverage discourages plans to retire before age 62 for the obvious reason that one can ordinarily qualify for benefits only at 62 or later. Even private pension coverage in addition to Social Security will not be expected to encourage retiring earlier. In fact, the data show that both those who expected only a private pension and the small group who expected no pension at all were more likely than others to plan to retire before 62. These are household heads, remember, not secondary earners or wives, and female heads were no more likely than others to plan very early retirement.

Working for a government encouraged very early retirement, a reflection of the generous levels of many government pensions and the lack of an earnings test for some. A high hourly wage also encouraged very early retirement, with the parabolic effect turning down the probability only at rather high wage rates ($57 an hour). Obviously, there is little "substitution" or "price of leisure" effect, and even adjusting for pension coverage and house value does not remove the effect of wages in encouraging very early retirement, presumably through its effect on expected retirement income. Notice that the regression even included a response about the expected adequacy of retirement income if the respondent were to retire at age 65.

Table 3.9

EXPECTED RETIREMENT AGE BY OCCUPATION AND WORK FOR GOVERNMENT
(Dividing occupations with substantial numbers of both
government and non-government workers.)
(For 1066 who have been family heads since 1972 and who were working in 1978.)

Occupation	Percent of Sample	Percent Who Say They Work for Government	Percent Planning to Retire: Before 62	62-64	65	66-71	72 or Later
Teachers	2	87	35	27	25	0	13
Technicians and protective service	3	39	42	25	20	0	13
Other professionals, non-government	8	0	19	13	41	6	22
Other professionals, government	3	100	22	26	42	0	10
Employed managers, non-government	13	0	17	17	48	9	9
Employed managers, government	4	100	33	8	20	0	39
Self-employed managers	4	3	19	24	14	0	43
Clerical, non-government	6	0	29	13	39	7	13
Clerical, government	3	100	35	13	27	17	9
Sales	4	0	31	9	43	3	13
Foremen	5	11	31	27	34	3	5
Craftsmen, non-government	14	0	14	29	34	1	23
Craftsmen, government	3	100	26	24	29	0	21
Transport, operatives	4	11	24	22	17	6	31
Non-transport operatives	11	5	12	35	28	4	21
Laborers	4	21	23	25	19	1	33
Service workers	8	26	6	20	34	2	37
Farmers	3	1	5	22	21	0	52
All 1066 same heads since 1972, not retired and working		19	20	22	33	4	22

For 1,066 who have been family heads since 1972 and who were working in 1978.

On the other hand, the systematic if insignificant pattern of effects of changes in wage rate would indicate that decreases in real wage encouraged very early retirement and increases discouraged it, the latter perhaps representing a

Table 3.10 (page 1 of 2)

REGRESSIONS FOR AGE OF EXPECTED RETIREMENT
(For constant family heads 1972-1978.)

Descriptive Variable	Before 62	Before 65 If Not Before 62	At 65 Rather Than Later	Before 72 If Not Before 66
Head's hourly wage	.021	.010	-.007	-.006
	(.006)**	(.005)	(.005)	(.005)
Wage squared/100	-.037	-.012	.011	.010
	(.016)*	(.007)	(.007)	(.007)
Real wage 77/72, <.81	.033	-.082	-.029	-.054
	(.043)	(.053)	(.059)	(.053)
Real wage 77/72, .81-.95	.056	-.145	.034	-.000
	(.040)	(.052)**	(.059)	(.052)
(Real wage 77/72, .96-1.04)	-	-	-	-
Real wage 77/72, 1.05-1.19	-.022	-.115	.019	-.063
	(.040)	(.051)*	(.060)	(.053)
Real wage 77/72, 1.20+	-.020	-.165	-.037	-.054
	(.037)	(.047)**	(.053)	(.047)
Self-employed***	.011	.111	-.243	-.268
	(.040)	(.049)*	(.052)**	(.047)**
Works for government***	.096	.037	-.007	-.033
	(.033)**	(.043)	(.047)	(.043)
Union job***	.083	.089	.095	.091
	(.032)**	(.041)*	(.047)*	(.042)*
Social Security only	-.371	-.045	-.061	.003
	(.058)**	(.100)	(.103)	(.093)
Both Social Security and other pensions	-.432	.064	.027	.065
	(.053)**	(.100)	(.102)	(.091)
(Other pension only or neither)	-	-	-	-
Unemployed 1-15 weeks, 72-77	-.056	.016	-.032	-.062
	(.038)	(.047)	(.055)	(.049)
Unemployed 16 or more weeks, 72-77	-.036	-.033	-.085	-.141
	(.038)	(.047)	(.050)	(.045)**
Not unemployed 71-77	-	-	-	-

rising opportunity cost and rising living standards that might be desirable in retirement.

The few respondents who expected to have mortgage payments to make at age 65 were less likely to plan very early retirement, as were those with a large house. A family head with a working wife four or more years younger was also discouraged from very early retirement. On the other hand, having a wife who expected a private pension of her own apparently encouraged very early retirement. Just

Table 3.10 (page 2 of 2)

Descriptive Variable	Before 62	Before 65 If Not Before 62	At 65 Rather Than Later	Before 72 If Not Before 66
Ill 1-7 weeks, 72-77	.019	.069	.089	.070
	(.031)	(.030)	(.041)*	(.037)
Ill 8 or more weeks, 72-77	.118	.084	.044	.048
	(.036)**	(.045)	(.040)	(.044)
Not ill 72-77	-	-	-	-
Still have mortgage payments at 65***	-.178	-.076	-.154	.059
	(.088)*	(.081)	(.087)	(.077)
House value/ 10,000	.011	.005	.000	-.002
	(.004)**	(.005)	(.005)	(.005)
Working wife 4 or more years younger	-.066	-.007	.065	.060
	(.034)*	(.041)	(.045)	(.041)
Wife expects a private pension	.105	-.010	.073	.087
	(.035)**	(.047)	(.052)	(.046)
Job enjoyable in 72 (5 pt)	-.036	.004	-.030	-.025
	(.014)**	(.017)	(.020)	(.018)
Would work even if had money (76)***	-.040	-.159	.026	.027
	(.026)	(.033)**	(.037)	(.033)
Expects adequate retirement income at 65***	.036	.028	.018	.006
	(.011)**	(.013)*	(.015)	(.013)
Single man* **	.103	.159	.086	.033
	(.053)*	(.073)*	(.085)	(.076)
High school graduate***	.126	.169	.066	.030
	(.033)**	(.042)**	(.050)	(.045)
Manager	-.063	-.057	.021	.057
	(.033)	(.041)	(.043)	(.039)
Operative	-.023	.080	-.064	-.031
	(.036)	(.045)	(.051)	(.046)
(Neither manager nor operative)	-	-	-	-
Plans ahead	.115	-.031	.021	.047
	(.025)**	(.031)	(.034)	(.031)
N	979	883	663	663
R^2 (adj.)	.19	.10	.08	.10

* significant at .05 level
** significant at .01 level
***Excluded group is everyone else
Note: Omitted groups in dummy variable sets are given in parentheses.

having a working wife seemed to have no effect on retirement, perhaps because these two offsetting effects hid one another. Illness encouraged plans for very early retirement, but unemployment worked the other way (insignificantly, but systematically over all of the decisions).

Single men were more likely to plan very early retirement, perhaps because they could get along on less money. High school graduates (that is, persons with neither more nor less education) were more likely to plan very early retirement, even taking account of all the other factors in the regression.

Finally, three more judgemental variables affected very early retirement: Reporting (in 1976) that one planned ahead was associated in 1978 with planning very early retirement. Reports in 1972 that the job was enjoyable led to fewer very early retirement plans in 1978. Finally, expecting an adequate retirement income at age 65 was associated with planning to retire even before 62.

The expectation about adequacy of retirement income appeared in the model as an intermediate variable, between the background and current situation variables on the one had and retirement expectations on the other. However, it does not either add much to the total explanation nor change the other coefficients much. The multiple r-squareds adjusted are:

Expecting an adequate retirement income, and planning early retirement	.029
Everything except expecting an adequate retirement income and planning early retirement	.185
Everything, and planning early retirement	.194
Everything else, and expecting an adequate retirement income	.155

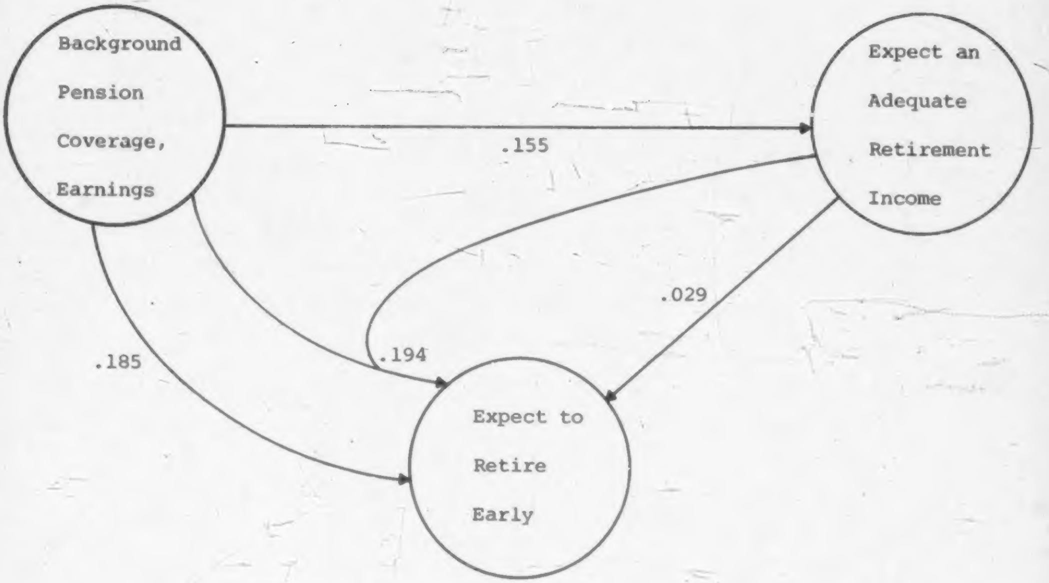
(See Figure 3.4.) The same orders of magnitude appeared for the other retirement decisions except that all the correlations were lower.

Plans to Retire Before 65, If Not Before 62

For those who did not plan to retire before age 62, we can examine factors affecting decisions to retire before 65, as a third of that group planned to do. We can explain only half as much of the variance we were able to explain for decisions to retire before 62. Except for the effects of having a union job, being a single man or a high school graduate, and expecting an adequate retirement income at 65, which continued to encourage retirement, a quite different set of predictors emerged in this portion of our analysis. The self-

Figure 3.4

PATTERN OF VARIANCE EXPLAINED
(Multiple r-squares, adjusted.)


Background Pension Coverage, Earnings
Expect an Adequate Retirement Income
Expect to Retire Early
.155
.029
.194
.185

*The squared correlation between expecting an adequate retirement income at 65 and planning to retire before 65 is .029.

employed were more likely than other workers to plan to retire before 65, but they were also more likely to plan to retire after 65 or after 72--there was no magic in 65 for them.

Wage levels had no effect on this decision, but the pattern of recent change in wage showed two non-symmetrical effects: a falling real wage discouraged early planned retirement, presumably because of need for more accumulation of savings and pension rights; an increase in real wages also discouraged plans for early retirement, presumably because of the rising opportunity cost. (The price of leisure has risen faster than the alternative pension benefits.)

Plans to Retire at 65 or Before 72 for Those Not Planning to Retire Before 65

Fewer cases remain for this analysis (663 respondents), and there was a more disproportionate division to explain. Of those not planning to retire before 65, 74 percent planned to retire at age 65 and 81 percent said they would retire before age 72. Only the self employed were substantially less likely to plan retirement at 65. Unemployment discouraged plans to retire at 65, but insignificantly, and illness encouraged retirement at 65 rather than later.

It should be noted that for each of the conditional plans, with only one exception out of 16 coefficients, extensive unemployment was a significant factor discouraging plans to retire even before age 72, and illness greatly encouraged retirement before 62. It seems clear that people view unemployment as a temporary problem to which they respond by working additional years to recoup their losses, while they see illness as foreshadowing further difficulty and think of retiring while there is still time to enjoy life.

Summary

There is indeed something that can be called retirement, which occurs relatively rapidly when poor health demands it or adequate retirement income allows it. Pensions other than Social Security, whether because they are more adequate or have less stringent earnings tests, are more likely to encourage early retirement. And even the small group of respondents who expected no pensions at all were more likely to plan to retire very early than were those with Social Security.

Factors affecting the retirement decision operate differently at the different retirement ages. Planning to retire very early, before age 62, was more frequent among those who had been ill, those whose wives expected pensions of their own, those with good homes with no mortgage payments by age 65, those who worked for the government, those whose jobs were not enjoyable, high school graduates who claimed to plan ahead, and those who had a union job at a high wage.

Planning to retire between ages 62 and 65 was more likely for those whose wages just kept up with inflation. Those whose wages had not kept up presumably felt they could not afford to retire. Those whose wages had risen faster than inflation may not have wanted to give up the opportunity for those high earnings. This was the only case where we were able to see any evidence of a marginal substitution between leisure and money. For the most part, income effects dominated. Respondents who said they would not work if they didn't need the money (in 1976) were also more likely to plan (in 1978) to retire before age 65.

Retiring at 65 rather than later was affected, but not significantly, by some of the same factors. The self-employed were less likely to plan to retire at 65 than at some age before or after, presumably because they had no pension plans that specified a 65 retirement age. Finally, just as illness encouraged very early retirement, unemployment encouraged very late retirement.

There are important implications of these findings. If retirement is dominated by health and by available retirement income relative to some normative standard, then forecasting aggregate dynamics of the labor force and of the flow of funds into and out of the Social Security system demands that we focus on levels of likely pensions, including multiple coverage, and on how fast people's aspiration levels rise, rather than on substitution effects. Indeed, the one marginal substitution effect that appears effective is that of high hourly earnings which rise faster than inflation in encouraging people to continue to work in spite of advancing age.

A Postscript

The true enormity of the inflation may well not have hit by spring 1978, and in the 1979 reinterviews everyone in the panel was asked how inflation was affecting them, and in particular whether inflation had caused them to change their ideas about retirement. A substantial minority appear from early analysis to report that they were thinking of delaying retirement, or among the retired to regret that they had retired.

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Chapter 4

AUTHORITY AT WORK: HOW MEN AND WOMEN DIFFER

Martha S. Hill

INTRODUCTION

In recent years substantial attention has been directed toward observed sex differences in the work setting. Of primary concern to economists interested in this area has been the wage gap between men and women--a persistent gap, with women's average wage level amounting to only about 60 percent of men's average wage level. Studies by economists (Corcoran and Duncan, 1979, Mincer and Polachek, 1974; Malkiel and Malkiel, 1973, Oaxaca, 1973; Cohen, 1971; Fuchs, 1971) have found that productivity-related characteristics--education, training, experience, and labor force attachment--account for, at most, half of the observed wage differential. Other studies, by both economists and sociologists, suggest that an important additional source of the sex difference in wages is related to the characteristics of the jobs held by men and women.

While research on job characteristics has primarily focused on occupational status, recent studies (Hill and Morgan, 1979; Wright and Perrone, 1977) indicate that specific characteristics of jobs are better predictors of wages than is occupation. In particular, these studies suggest that wages are strongly affected by the worker's position in the work hierarchy.[1] That this characteristic of the job plays a role in sex differences in wages is supported by Halaby's (1977) finding that, among men and women working in the same occupation for the same employer,[2] differences in rank position accounted for 65 percent of the sex differences in salaries, whereas differences in productivity-related characteristics accounted for only 27 percent of the sex differences in salaries.

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[1] Hill and Morgan (1979) found that characteristics of the job are considerably better predictors of wages than is a 15-category specification of type of occupation. Among these characteristics are two dealing with the worker's position in the authority structure--one representing the worker's supervisory responsibilities and one representing the worker's say over others' pay or promotion. They found that having these responsibilities increases wages by 20 percent, even with occupation included as a wage predictor along with numerous worker qualifications. Wright and Perrone (1977) found that, among individuals working at least 35 hours per week, a three-category Marxian class typology--dividing the sample into employers, managers, and workers--is at least as strong a predictor of earnings as the full Duncan socioeconomic scale.

Yet while substantial work has been done on the process of attaining occupational status, relatively little has been done on the process by which workers attain positions in the hierarchial job structure. The literature thus far on this topic consists of two works, Wolf and Fligstein (1979) and Halaby (1977), both of which involve rather limited samples. Since the position attained in the hierarchial structure affects wages, further understanding of this attainment process for both men and women may shed additional light on the processes generating sex differentials in wages. The present analysis focuses on authority attainment in the work place and examines differences in the attainment processes of men and women using national samples of working, non-self-employed adults.

BACKGROUND

Extensive work has been done on the occupational status attainment process for men, and in recent years the occupational status attainment process for women has received more attention. Research in this area has indicated, on a cross-sectional basis and in terms of occupational status or prestige, that men and women are quite alike. Treiman and Terrell (1975) found similar means, variances, and distributions on occupational prestige for men and women; McClendon (1976) found comparable results using Duncan's socioeconomic index as the measure of occupational status. These authors, along with Featherman and Hauser (1976), also found cross-sectional similarities in the occupational attainment processes for men and women; for both sexes, education was shown to exert a positive effects of similar magnitude and considerably stronger than the effects of family backgound factors.

Yet, despite these cross-sectional similarities in occupational status, men and women differ considerably in their occupational distributions and their patterns of occupational achievement over the life cycle. Hill and Morgan (1979), Treiman and Terrell (1975), and Oppenheimer (1970), among others, have documented the high concentration of women in certain occupations. The proportions of women workers in managerial, craft, and laboring work are exceptionally low, whereas the proportions of women workers in clerical, certain types of professional (teaching, nursing), and service work are exceptionally high. Additionally, McClendon (1976) noted that cross-sectional sex equality in occupational status is produced by a high concentration of women in white-collar jobs (which rank higher in status than blue-collar jobs), combined with a tendency for women to occupy lower status jobs than men within both white-collar and blue-collar jobs. Also, while Duncan, Featherman, and Duncan (1972) found that men's occupational status tends to increase over the life cycle, especially early in their work lives, Wolf and Rosenfeld (1978) reported having found that women's occupational status remains essentially constant. Further, as noted in the introduction, it is well-established that women earn less than men, even when they hold jobs of equal status.

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[2] Halaby's sample consists of management personnel in a large U.S. public utility holding company.

Perhaps a less well documented but nonetheless persistent finding is that women tend to rank lower than men in the power structure of the work environment (Halaby, 1977; Wolf and Fligstein, 1979). While this aspect of sex differences reflects differences in social position, it can influence economic position as well (Hill and Morgan, 1979; Halaby, 1977; Wright and Perrone, 1977).

Some research has addressed the issue of factors associated with position in the work heirarchy. Wright and Perrone (1977), with their Marxian class typology, found that "managers" experience greater wage returns to education than "workers" and suggested that position in the managerial hierarchy depends strongly and positively on education because: (1) higher education produces (or at least certifies) the skills required for higher positions in the work hierarchy; (2) higher education socializes individuals to the work habits appropriate for higher levels of authority; and (3) education, through an ideology of rule by experts, legitimizes inequalities of power.

Halaby (1977) looked more directly at factors associated with position in the work hierarchy. Although his primary focus was on wage differences between men and women, he also predicted the level of rank obtained by workers in the same occupation with the same U.S. firm. He found that education does indeed have a strong positive effect on rank in the hierarchy but that the effect is much stronger for men than for women. Other predictors included in his analysis were measures of years with the firm, years of work experience prior to entering that firm, and number of positions held in other firms prior to entering that firm. Of these predictors, only seniority attained significance at the .05 level or higher and then only among women; however, the coefficient on this predictor was positive and of roughly the same magnitude for both men and women. Decomposing the difference between the average rank of men and women, he found that differences in the returns to schooling were most strongly associated with rank differences. The effects of returns to schooling were much stronger than the effects of returns to experience and also much stronger than the effects of overall levels of education or experience.

The primary work in the area of sex differences in positions in the work hierarchy has been undertaken by Wolf and Fligstein (1979). Examining employees in different occupations, these authors focused on three aspects of position of authority in the work setting--responsibilities to hire and fire, to have say over others' pay, and to supervise others. They treated each of these aspects as a dichotomous dependent variable in regressions run separately for men and women. While their sample (of Wisconsin high school graduates, approximately 27 years of age) presents some problems in terms of generalizabilty of their results, their study has been a path-breaking contribution to an area that has received little research attention.

Including as predictors such productivity-related characteristics as education, experience, and tenure with the current employer, along with familial characteristics (marital status and presence of children) and job characteristics, they found substantial differences between men and women in the way workers attain positions of authority. Like Halaby, they found that men tend to receive considerably higher returns to education in the form of increased likelihood of attaining authority. They also found differential effects of experience, tenure, being a parent, and job characteristics. Experience and tenure showed positive effects in the regressions for women but no significant positive effects in the regressions for men; in fact, the only significant effect of these variables in the regressions for men was a negative coefficient on tenure in a regression on responsibility to hire or fire. While marital status showed no significant effect for men or women, being a parent affected only men, exerting a positive effect on all types of supervisory responsibilities. And, while the job characteristics--occupational (socioeconomic) status and the degree to which the occupation is male-dominated--mediated the effects of other variables for both men and women, they were shown to exert stronger independent effects on men's attainment of authority.

Decomposing the mean sex differences in the values of the three measures of authority on the basis of their regression results, Wolf and Fligstein further found that while women's inferior qualifications account for some of the difference in authority, a much larger proportion of the difference is due to differential returns to qualifications, particularly in the case of the upper level types of authority. Adding the occupational measures, they also found that differential returns to occupations rather than differential membership in occupations account for much of the sex differences in authority. They tentatively interpreted this differential treatment of men and women as an indication that it is more likely to be the behaviors and policies of employers rather than their own attitudes and behaviors that prevent women from attaining positions of authority.

RELEVANT THEORY

In general, the literature indicates that while men and women in the work environment are similar with respect to occupational (socioeconomic) status, their wages, type of occupation, and hierarchial rank tend to differ. In searching for explanations for any one of these differences, the co-existing differences should be considered. Thus in searching for theories relevant to sex differences in positions of authority at work, we review theories relevant to occupational segregation and wage differentials.

Anthropological and some sociological studies have searched for the cultural roots of differences in the labor market roles of men and women and, in general, have concluded that the early development of sex-related division of labor in the home is the major basis for differential placement in the labor market. (See, for example, Hartman, 1976; Bernard, 1976; and Boulding, 1976.) These studies have been concerned with societal forces that generate these phenomena. Economists interested in this topic, on the other hand, have been primarily concerned with the role of market forces in the generation of differential labor market placement and treatment. Several economic theories have been developed to account for the observed differences.

Two economic theories dealing with these topics are based on neoclassical economics. One of these approaches is human capital theory. Developed primarily as an explanation for wage differentials, this theory argues that stocks of human capital (skills developed through training, education, and experience) determine the value of the worker in productive processes and thus the wages a worker receives in the labor market. The human capital explanation for sex differentials in earnings is that the lower wages of women are the result of lower stocks of accumulated capital resulting from intermittent labor force participation, particularly in the early stages of career development.

Although occupational segregation is not necessarily a natural consequence of labor markets operating in accordance with the human capital theory, the human capital approach can be used to explain the existence of occupational segregation as well. Intermittent labor force participation not accompanied by additional schooling or other forms of increased training decreases an individual's chances of augmenting his or her stock of human capital. Since many women work intermittently, with spells out of the labor market for family reasons, they may choose to enter occupations that do not penalize them severely for discontinuous employment. For the same reason they may tend to avoid occupations that require lengthy training periods in favor of occupations that offer flexible work hours and widespread opportunity for employment, since familial obligations may limit them to certain work hours or geographic locations.

Another economic theory based on neoclassical economics pertains more directly to occupational segregation. This theory, known as the overcrowding hypothesis, was first espoused some 50 years ago by economists such as Fawcett and Edgeworth and adopted more recently by Bergmann. This hypothesis, like human capital theory, asserts that wage differentials are the result of differences in productivity, but it cites a different source for them--exclusionary behavior by either employees or employers. According to the overcrowding hypothesis, women earn less than men because they are confined to certain types of occupations, thus increasing the supply of workers in these occupations, which in turn exerts downward pressure on their wages and upward pressure on the wages of others (mainly men) in occupations not open to them. As with human capital theory, workers are assumed to earn the value of their marginal products, but, since the capital-labor ratio in crowded occupations is lower than that in uncrowded occupations, workers in the crowded occupations receive lower wages. According to Edgeworth (1922) and Fawcett (1918) the factor generating the exclusionary practice in the 1920s was male-dominated unions. According to Bergmann (1974, 1971), a demand factor--employer's tastes--operated in the 1970s to exclude women from certain types of occupations. Bergmann's version of the overcrowding hypothesis, as pointed out by Blau and Jusenius (1976), is consistent with Becker's (1957) theory of discrimination and relies on widespread feelings among employers that women should not hold certain positions, even though the existing wage differentials offer an economic incentive to hire women instead of men in male-dominated occupations.

A competing theoretical approach to both differentials in wages and occupational segregation relies on institutional explanations for these phenomena. According to some proponents of institutional explanations (see, for example, Doeringer and Piore, 1971), jobs in the labor market fall into two different categories--those in the "primary" sector and those in the "secondary" sector. Jobs in the primary sector are characterized by opportunities for advancement (including, presumably, the opportunity to attain authority), good working conditions, high wages, and provisions for job security. Jobs in the secondary sector, on the other hand, offer few chances for advancement, poor working conditions, low wages, and little job security. Additionally, jobs in the primary sector involve a strong internal labor market, in which the wages and promotional possibilities for individuals are determined by their job assignments in accordance with rigid rules governing the wages and promotional ladders established for each type of job. The secondary labor market is presumed to involve a set of unstructured markets, with more arbitrary administration of work rules.

Wage differentials and occupational segregation can result from this labor market segmentation. Because jobs in the primary sector tend to require employers to invest in the development of an employee's skills that are specific to his or her job assignment, employers seek stable employees with low absenteeism and low turnover rates to fill these jobs. In the screening process, employers may rely on probabilistic factors, basing the employment decisions for an individual on group-derived probabilities rather than individual factors that are more difficult to ascertain (Spence, 1974; Thurow, 1975, Phelps, 1972). Noting the tendency for women as a group to have lower labor force attachment than men, employers may categorically exclude women from jobs in the primary sector.

As pointed out by Rosenfeld (1979), these institutionally based hypotheses presume that the job location in the labor market is the primary factor determining wages and chances of advancement rather than characteristics of the job holder, although sex may be a factor in determining labor market location. The work by Rosenfeld (1978) lends some support to this contention; however, Halaby (1977) found that although men and women in the same occupation within the same firm began their careers at similar levels, men advanced further up the ranks.

In drawing implications of these theories with regard to factors affecting a worker's position in the job hierarchy, we rely on the assumption that determination of position in the authority structure operates in a manner similar to wage and occupation determination. In this regard, human capital theory would tend to predict that individual characteristics such as education, employment history, skills, and individual tastes regarding desired labor force attachment would be the major factors affecting an individual's position in the job hierarchy. These characteristics should have the same effects on every worker's chances of having authority over other workers, whether the worker is a male or a female. The institutional theories, however, would tend to predict that obtaining entry to a job that required substantial specific training, whether completed or yet to be completed, should enhance the possibilities of having authority in the work setting and that this effect of training associated with the job should be the same for men and women. The overcrowding hypothesis, on the other hand, would tend to predict that women would be excluded from positions of authority, not because of differential qualifications, but because employers or unions prefer to exclude them. With the exception of the overcrowding hypothesis, the implications of these theories are testable with adequate data obtained from the workers themselves.

DATA

The Panel Study provides a rich assortment of measures of workers' qualifications and labor force attachment. In addition, it provides measures of characteristics of the jobs such as occupation category, union status, and amount of training needed to become fully trained or qualified for the jobs, plus measures of information and influence networks used in getting the jobs. Most crucial to the present analysis, it provides measures of the workers' positions in the authority structure.

In order to take advantage of this rich collection of measures to study how men and women attain authority, however, certain restrictions have been placed on the two samples--one a subset of the other--that are used in the ensuing analysis. Only in 1976 were both employed household heads and employed wives asked questions pertaining to position in the authority structure, to a worker's qualifications and labor force attachment, and to training requirements Consequently, the ensuing analysis pertains to 1976 and the samples were restricted to Panel Study individuals who were either household heads or wives and employed in the spring of that year. Only individuals aged 18-64 in 1976 were included in the analysis. Additionally, the samples were restricted to

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[3] This information for wives was ascertained directly from the wives themselves.

workers who were not self-employed. As Wolf and Fligstein (1979) argued, the authority attainment process of self-employed workers is likely to differ substantially from that of employees, and labor market theories tend to be more applicable to employees.

A set of questions dealing with information and influence networks used in getting their current jobs was asked in 1978; these questions pertained to both employed household heads and their wives under age 45.[4] In order to include these measures of the job acquisition process in the analysis of the authority attainment process, the sample was further restricted to Panel Study individuals who were employed household heads or wives in both 1976 and 1978, who were under age 45 in 1978, and who worked for the same employer in both 1976 and 1978.[5] This restricted sample was merely a subset of the broader sample.

We first tested the authority attainment model on the more restricted subsample, with the measures of the job acquisition process included. It was then applied to the larger, more comprehensive sample, with the measures of the job acquisition process excluded from the analysis.

Authority Attainment Model

A worker's position in the hierarchial job structure can be measured both in terms of autonomy--control over one's own work--and in terms of authority--control over others' work (Wolf and Fligstein, 1977). The present analysis focuses only on authority over others. This authority can be decomposed into supervisory responsibilities, which reflect authority in every-day work processes, and responsibilities for the placement of other workers, which reflect authority that is generally used less frequently but is at a higher level than supervision. We focus here on a measure of authority that reflects both aspects.

The dependent variable in this analysis is a dichotomous variable which takes on a value of 1 for workers who reported that they had say over the pay or promotion of the other employees that they supervised.[6] This variable combines

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[4] This information for wives was ascertained from their husbands.

[5] Specifically, this later restriction limited the subsample to individuals who reported working for their 1978 employer for at least two years.

[6] That is, the dependent variable had a value of 1 when the worker gave a "yes" response to the second question in the following sequence:

"Do you supervise the work of other employees, or tell them what to do?"

YES   NO (SKIP NEXT QUESTION)

↓

"Do you have any say about their pay or promotion?"   YES   NO aspects of authority that Wolf and Fligstein (1979) analyzed separately--responsibilities for hiring or firing, say over other's pay, and supervisory powers; in essence, it measures the likelihood of having authority over both the work and the position of other employees. Since the former aspect of authority generally accompanies the latter, the dependent variable is labeled simply "whether has say over others' pay or promotion."

Drawing primarily upon the implications of human capital and institutional theories and the work of Wolf and Fligstein, the present analysis explores the role of several types of variables on authority attainment. Included as predictors of "whether has say over others' pay or promotion" are characteristics of the worker, characteristics of the job, and characteristics of the networks used by the worker in acquiring the job.

Characteristics of the worker include:

Skills Measures:
- Years of education ("education" refers to formal education)
- Years of training completed ("training" refers to training needed for the present job)

Work Experience Measures:
- Pre-employer experience (years worked since age 18 minus years worked for present employer)
- Years with present employer

Indicators of Labor Force Attachment:
- Fraction of years worked full-time
- Years not working (since age 18 or since completing school if education is more than 12 years)
- Whether full-time worker (35 or more hours of work per week, on average, in present job)
- Hours of work missed due to illness of others in 1975
- Hours of work missed due to own illness in 1975
- Whether placed limits on job hours or location (when searching for the present job)
- Whether plans to stop work soon ("soon" refers to the next few years)
- Whether has disability that limits work (limits either the type of work or the amount of work)

Family Situation:
- Marital status
- Whether has children

Background:

Father's expected position in the authority structure based on father's occupation[7]

Race

In the authority regressions, squared terms for the work experience measures and fraction of years worked full-time were also included to allow for non-linearities. The latter variable could be classified as a work experience variable as well as an indicator of labor force attachment.

Characteristics of the job include:

Training Requirements:

Years of training not yet completed (this equals any positive residual from subtracting years in current position from years of training needed to become fully qualified for the present job)

Union Status:

Whether job covered by union contract

Occupation:

Whether occupation is teacher (primary or secondary but not college)

As listed above, "training requirements" consist only of training that is needed to become fully qualified for the job but is not yet completed. However, the regressions contain both this variable and the training variable listed under skills of the worker--years of training completed--and together they measure the amount of training associated with the job. These two variables were included separately, rather than aggregated, to allow tests of implications of both the human capital and insitutional theories. The other characteristics of the job were included for a different reason. Both teaching and union jobs entail rather unique "chains of command." To gain authority over other employees' pay or promotion, a teacher will often change occupational titles, becoming an administrator such as a principal, and union contracts generally divide the employees into "workers," who are in jobs covered by the contract, and "managers," who have authority over the workers covered by the contract but are not themselves covered by it. _A priori_ expectations are for negative effects of these variables.

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[7] This variable is constructed on the basis of results obtained by Hill and Morgan (1979) concerning the proportions of male workers reporting having say over others' pay or promotion in 1976 by occupation. They found that these proportions were exceptionally high for managers, foremen, and farmers and exceptionally low for operatives, laborers, primary and secondary school teachers, and service workers. To the extent possible the variables on father's expected ranking in terms of authority reflect these findings. Working with a one-digit rather than a two digit code for father's occupation (Hill and Morgan were able to use a two-digit code), individuals were classified as having a father with: high expected ranking if the father's occupation was manager or farmer, low expected ranking if the father's occupation was operative, laborer, or service worker, and intermediate ranking if the father's occupation was another type. With the one-digit code, foremen could not be distinguished from craftsmen and teachers could not be distinguished from other professional workers.

Characteristics of the job acquisition process include:

> Information network used in learning of the job opening (hearing about the job from a friend, relative, want ad, employment agency, etc.)
> Influence network used in acquiring the job (who helped them get the job, if anyone, and how much influence they probably had)

According to Marxian economists (Wright and Perrone, 1977), education and training are credentials that are used to legitimize authority at work; although they may or may not be skills that are needed to use the authority properly, they should have a positive effect on authority attainment. Human capital economists would argue that education and completed training are skills that are needed for positions of authority; they would tend to expect positive authority effects for these qualifications--positive effects of similar magnitude across workers, regardless of sex. Proponents of institutional explanations on the other hand, would argue that it is the training characteristics of the job rather than the worker that establish the chances of the job entailing authority over other workers; jobs with substantial training requirements are in the primary sector and thus entail opportunities for advancement to higher ranks of authority, whereas jobs requiring little training are the "dead-end" secondary sector jobs. Since it is the job rather than the person that determines the chances of authority attainment, and both training completed and training still needed to become fully qualified for the job reflect the training requirements of the job, proponents of institutional explanations would tend to expect similar effects for both training completed and training not yet completed; they would also tend to expect similar effects of training for men and women.

Economists in these different schools would also tend to differ in their expectations regarding the sex-specific effects of tenure and labor force attachment on chances of authority attainment. Human capital proponents would tend to expect similar effects for men and women, arguing that specific training may be needed for positions of authority and that an employer's rational decision would be to make these investments only in employees, men or women, who show potential for long-term commitment to the firm--those with substantial tenure and indication of a strong labor force attachment. Proponents of institutional theories, on the other hand, would expect weaker effects of these variables for women, arguing that women tend to be relegated to secondary-sector jobs that neither reward nor penalize workers for their degree of attachment to the firm or to the labor market in general.

The family situation, background, and job acquisition variables were included primarily for exploratory purposes. The family situation variables could register effects of either the worker's level of motivation or the employer's attitudes. Men with greater family responsibilities may be more likely to put forth the work effort and commitment to the firm needed to move up the hierarchial ladder to higher levels of authority, with accompanying higher levels of pay and consequently greater financial security for their families; women with greater family responsibilities, on the other hand, may be less likely to put forth this work effort and commitment either because of the time and energy required in taking care of their families or because of a weaker attachment to the labor force. Many of these effects, however, should be captured by the variables which measure labor force commitment more directly. It seems plausible that, with direct controls for labor market commitment, the family situation variables would pick up effects of employer's attitudes. Forming and raising a family may be viewed by employers as an indicator of authority potential--a positive one for men and a negative one for women.

The background and job acquisition variables were included to try to detect the way that sources of information and/or influence operate in the authority attainment process--to see if particular means of acquiring information about jobs or having influential connections affect the chances of a worker acquiring a position of authority.

The authority attainment model consists of specifying these characteristics--of the worker, of the job, and of the job acquisition network--as predictors in regressions on the dichotomous variable "whether has say over others' pay or promotion." Separate regressions were run for men and women,[8] and, on the basis of these results, as well as those of a regression combining men and women and specifying all possible sex interactions, tests were made for whether the authority attainment process differs by sex. Three sets of male/female regressions were performed. As mentioned earlier, the job acquisition variables were measured only for a subset of the sample; consequently the first set of regressions, which included these variables as predictors, were restricted to this subset. The second two sets of regressions, which did not include the job acquisition variables, were run for both the subset and the entire sample.

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[8] Since a preliminary search of the data with a formal search procedure (with OSIRIS IV's "Search") indicated substantial sex differences in the authority attainment process but no substantial race differences, the sample was divided only by sex and not further by race.

EMPIRICAL FINDINGS

Before discussing the regression results, a comparison of the relative standings of the men and women in our samples is in order. Table 4.1 shows that, while similar proportions (about 80 percent) of the men and women reported that their boss had a boss, men and women differed considerably in terms of the proportions with authority over others' work or position at work. In both the larger sample and the subsample, a much higher proportion of men than women had say over others' pay or promotion--22.9 percent for men as opposed to 10.3 percent for women in the subsample of younger workers and 25.5 percent for men as opposed to 9.7 percent for women in the sample of all workers. Similarly, the proportion supervising others was about twice as large for men.

Having say over others' pay or promotion denotes higher authority than mere supervisory power. This is consistent with the drop in proportions as we move from having any supervisory power to also having say over others' pay or promotion and the negative correlation between "whether boss has a boss" and "whether has say over others' pay or promotion." The drop in proportions as we move up the authority scale, however, was sharper for women than for men. The means indicate that about half of the men but only about one-third of the women with supervisory power also had say over others' pay or promotion; also, the correlations between supervising and having say over pay or promotion were lower for women than for men.

An examination of the mean values on the variables to be treated as predictors of the likelihood of having authority over others' position in the work setting (Table 4.2) indicated that the sample of women did, in fact, tend to have lower qualifications and less labor force attachment than the sample of men. Although these men and women averaged the same number of years of education, the men averaged more years of completed job training, more years of pre-employer experience, and more years with the present employer. A larger proportion of the years of work experience for men were years of full-time work, and fewer of their potential work years were spent out of the labor force. Additionally, the men were more likely to be presently working full-time and less likely to plan to stop work in the near future and to have placed restrictions on work hours or job location. The men also averaged less absenteeism as a result of their own illness or the illness of others in their family.

Table 4.1

MEAN VALUES AND CORRELATIONS BETWEEN INDICATORS OF
POSITION IN THE WORK HIERARCHY, AS OF 1976 BY SEX
(For household heads and wives working for someone else in 1976.)

	Workers Aged 18-43, with Same Employer 1976-1978		Workers Aged 18-64	
	Men	Women	Men	Women
	Mean Value			
Whether boss has a boss	.818	.835	.813	.757
Whether supervises other employees	.449	.281	.460	.273
Whether has say over others' pay or promotion	.229	.103	.255	.097
Correlation with Whether has say over others' pay or promotion				
Whether boss has a boss	-.084	-.088	-.102	-.072
Whether supervises other employees	.603	.542	.634	.534
Sample size	1255	822	2723	2166

Table 4.2 also shows sex differences in family situation, job characteristics, and job information and influence networks used in getting the job. Although about the same proportions of working men and women were parents, somewhat larger proportions of the working men were married. In terms of their jobs, men were more likely to be in a job covered by a union contract and less likely to be a teacher in a primary or secondary school. In terms of networks used in getting the job, men were more likely to report having had help from a relative--help both in learning of the job and in influencing the hiring decision. Women were somewhat more likely to report having learned of the job from a friend and having had no help in influencing the hiring decision. Of those who received help in influencing the hiring decision, a larger proportion of men than women reported having received help from someone who worked there and could have had substantial say in who was hired.

Table 4.2 (Page 1 of 2)

MEAN VALUES ON INDEPENDENT VARIABLES USED IN THE ANALYSIS
(For household heads and wives working for someone else in 1976.)

	Workers Aged 18-43, With Same Employer 1976-1978		Workers Aged 18-64	
	Men	Women	Men	Women
Years of education	13.11	13.95	12.62	12.62
Years of training completed	1.25	0.71	1.48	0.66
Years of training not yet completed	0.53	0.24	0.54	0.23
Pre-Employer experience	5.95	5.04	10.07	7.77
Pre-Employer experience squared	61.69	47.00	191.37	122.4
Years with present employer	5.79	4.43	9.09	5.50
Years with present employer squared	56.76	38.14	163.17	70.18
Fraction of work years fulltime	0.88	0.80	0.91	0.78
Fraction of work years fulltime squared	0.83	0.71	0.86	0.70
Years not working	0.33	3.05	0.54	5.97
Whether fulltime worker	0.96	0.71	0.96	0.73
Hours of work missed due to illness of others in 1975	3.84	19.67	4.11	15.22
Hours of work missed due to own illness in 1975	62.46	73.41	77.67	77.84
Whether placed limits on job hours or location	0.14	0.33	0.15	0.34
Whether plans to stop work soon	0.01	0.06	0.04	0.10
Whether has disability that limits work	0.05	0.03	0.07	0.06
Whether married	0.84	0.71	0.85	0.68

Table 4.2 (page 2 of 2)

	Workers Aged 18-34, With Same Employer 1976-1978		Workers Aged 18-64	
	Men	Women	Men	Women
Whether separated, widowed, or divorced	0.05	0.14	0.07	0.21
Whether has children	0.68	0.63	0.58	0.51
Whether father's occupation ranks high in terms of authority	0.23	0.22	0.27	0.28
Whether father's occupation ranks intermediate in terms of authority	0.48	0.52	0.44	0.46
Whether black	0.10	0.13	0.10	0.11
Whether job covered by union contract	0.34	0.24	0.34	0.19
Whether occupation is teacher	0.03	0.12	0.03	0.09
Whether learned of job from relative	0.22	0.11		
Whether learned of job from want ad	0.08	0.12		
Whether learned of job from employment agency	0.06	0.07		
Whether learned of job from other source, but not friend	0.33	0.35		
Whether relative helped in getting job	0.14	0.08		
Whether someone other than relative or friend helped in getting job	0.04	0.06		
Whether received no help in getting job	0.63	0.70		
Whether direct influence of someone helped in getting job	0.07	0.04		
Whether employee with a lot of say helped in getting job	0.14	0.07		
Sample Size	1255	822	2723	2166

The background variables--race, and father's expected rank in terms of authority at work--registered no substantial mean differences by sex. Additionally, the proportions of men and women who reported having a disability that limited the type or amount of work they could do were quite similar.

Regression Results

Ordinary least squares regression is, in some respects, not an ideal method of analysis of a dichotomous dependent variable such as the one we are interested in. (See, for example, Goldberger, 1964, and Goodman, 1976.) Although ordinary least squares regression on a dichotomous dependent variable yields unbiased coefficient estimates, these estimates are inefficient. Additionally, it can yield estimated values for the dependent variable outside the 0 to 1 range and, as a result, can produce deceptively low R^2s. These qualities of OLS regression are particularly problematic when the mean value for the dichotomy falls outside the range of .1 to .9. As noted in the previous section, the mean values for the dependent variables in the present analysis do border on this range. Despite the possible problems, however, the analysis described below focuses on OLS regression results concerning the dichotomous dependent variable "whether has say over others' pay or promotion" since this analysis yields readily interpretable results, including ones not possible with techniques such as multivariate logit. The results of OLS regression allow us to identify the extent to which differences in mean values on the independent variables vs. differences in the effects of the independent variables play a role in the observed sex differences in the likelihood of having authority over other workers in the workplace. These OLS regression results, however, do not yield totally reliable significance tests.

Results of the sex-specific regressions for both the subset of younger workers with subsequent tenure of two additional years and the larger sample of all workers are presented in Table 4.3. Regressions for the subset include both a set that contains the variables representing the information and influence networks used in getting the job (Regression 1) and a set that does not (Regression 2). Since these variables are the only ones in the model that are not available for the larger sample, the latter regression is included for more accurate comparison of the authority attainment processes of the subset and larger sample. (Regression 3 involves a specification identical to that of Regression 2.)

Table 4.3 (page 1 of 3)

COEFFICIENTS AND STANDARD ERRORS FOR REGRESSIONS
WITH WHETHER HAVE SAY OVER OTHER'S PAY OR PROMOTION
AS THE DEPENDENT VARIABLE
(For household heads and wives working for someone else in 1976.)

	Workers Aged 18-43, With Same Employer 1976-1978				Workers Aged 18-64	
	Regression #1		Regression #2		Regression #3	
	Men	Women	Men	Women	Men	Women
Years of education	.025** (.005)	.007 (.006)	.024** (.005)	.006 (.006)	.032** (.003)	.012** (.003)
Years of training completed	.023** (.009)	.063** (.013)	.024** (.009)	.065** (.013)	.033** (.005)	.047** (.007)
Years of training not yet completed	.038** (.009)	.066** (.015)	.037** (.009)	.063** (.015)	.051** (.006)	.041** (.008)
Pre-Employer experience	-.0069 (.0065)	-.0127+ (.0066)	-.0087 (.0064)	-.0129* (.0066)	-.0038 (.0024)	.0003 (.0021)
Pre-Employer experience squared	.0006+ (.0003)	.0010* (.0004)	.0006+ (.0003)	.0010* (.0004)	.0002** (.0001)	.0000 (.0001)
Years with present employer	.0332** (.0073)	.0026 (.0075)	.0324** (.0072)	.0008 (.0074)	.0137** (.0029)	.0092** (.0024)
Years with present employer squared	-.0012** (.0004)	.0000 (.0004)	-.0011** (.0004)	.0001 (.0004)	-.0002* (.0001)	-.0001 (.0001)
Fraction of work years fulltime	.170 (.244)	.057 (.170)	.184 (.243)	.040 (.169)	.203 (.179)	.174+ (.092)
Fraction of work years fulltime squared	-.137 (.186)	-.084 (.137)	-.153 (.186)	-.072 (.136)	-.070 (.135)	-.137+ (.076)
Years not working	-.024* (.011)	-.002 (.003)	-.025* (.010)	-.002 (.003)	-.020** (.005)	-.000 (.001)
Whether fulltime worker	.135* (.039)	.078** (.029)	.132* (.059)	.079** (.028)	.087* (.039)	.038* (.015)
Hours of work missed due to illness of others in 1975	.0005 (.0007)	-.0000 (.0001)	.0004 (.0007)	-.0000 (.0001)	-.0002 (.0004)	-.0001 (.0001)
Hours of work missed due to own illness in 1975	-.0001 (.0001)	.0000 (.0001)	-.0001 (.0001)	.0000 (.0001)	-.0001 (.0000)	-.0000 (.0000)

Table 4.3 (page 2 of 3)

	Workers Aged 18-43, With Same Employer 1976-1978				Workers Aged 18-64	
	Regression #1		Regression #2		Regression #3	
	Men	Women	Men	Women	Men	Women
Whether placed limits on job hours or location	.002 (.032)	-.015 (.022)	-.000 (.032)	-.010 (.022)	-.028 (.021)	.014 (.013)
Whether plans to stop work soon	-.207+ (.107)	-.034 (.043)	-.204+ (.106)	-.034 (.043)	.006 (.040)	-.024 (.021)
Whether has disability that limits work	.034 (.055)	-.007 (.058)	.044 (.055)	.003 (.056)	.059* (.030)	.004 (.026)
Whether married	-.017 (.043)	.024 (.034)	-.018 (.043)	.038 (.033)	.030 (.031)	.026 (.022)
Whether separated widowed, or divorced	.089 (.061)	.042 (.042)	.087 (.061)	.060 (.042)	.088* (.039)	-.009 (.025)
(Whether single)	-	-	-	-	-	-
Whether has children	.055+ (.031)	.018 (.028)	.056+ (.031)	.026 (.027)	.045* (.018)	.000 (.014)
Whether father's occupation ranks high in terms of authority	.006 (.031)	-.021 (.030)	.010 (.031)	-.025 (.030)	-.009 (.020)	-.036* (.017)
Whether father's occupation ranks intermediate in terms of authority	.005 (.026)	.037 (.025)	.004 (.026)	.035 (.025)	.032+ (.018)	.008 (.015)
(Whether father's occupation ranks low in terms of authority)	-	-	-	-	-	-
Whether black	-.008 (.039)	-.033 (.033)	-.013 (.039)	-.036 (.032)	.010 (.026)	-.007 (.015)
Whether job covered by a union contract	-.213** (.025)	-.069** (.025)	-.214** (.024)	-.065** (.025)	-.217** (.016)	-.035* (.016)
Whether occupation is teacher	-.193** (.068)	-.233** (.040)	-.195** (.067)	-.224** (.040)	-.270** (.047)	-.207** (.026)
Whether learned of job from relative	.027 (.040)	.046 (.045)	-	-	-	-

Table 4.3 (page 3 of 3)

	Workers Aged 18-43, With Same Employer 1976-1978				Workers Aged 18-64	
	Regression #1		Regression #2		Regression #3	
	Men	Women	Men	Women	Men	Women
Whether learned of job from want ad	-.014 (.046)	.008 (.037)	-	-	-	-
Whether learned of job from employment agency	.004 (.051)	.042 (.046)	-	-	-	-
(Whether learned of job from friend)	-	-	-	-	-	-
Whether learned of job from other source	.006 (.031)	.029 (.028)	-	-	-	-
Whether relative helped in getting job	.023 (.051)	.051 (.057)	-	-	-	-
(Whether friend helped in getting job)	-	-	-	-	-	-
Whether someone other than relative or friend helped in getting job	.048 (.061)	.001 (.055)	-	-	-	-
Whether received no help in getting job	.001 (.034)	.042 (.034)	-	-	-	-
Whether direct influence of someone helped in getting job	.067 (.057)	.041 (.070)	-	-	-	-
Whether an employee with a lot of say helped in getting job	-.005 (.044)	.089+ (.053)	-	-	-	-
Constant	-.391	-.139	-.345	-.070	-.500	-.189
Sample Size	1255	822	1255	822	2723	2166
R^2 (adjusted)	.173	.111	.175	.107	.230	.089

**Significant at .01 level.
*Significant at .05 level.
+Significant at .10 level.

A simple comparison of the R^2s for these regressions with those obtained by Wolf and Fligstein (1979) suggests that the present analysis should add substantially to an understanding of the process of attaining authority, at least for men. While the regressions for men done by Wolf and Fligstein yielded R^2s unadjusted for degrees of freedom ranging from .052 to .144, the present analysis yields adjusted R^2s for men ranging from .173 to .230. Improvement in understanding of the authority process of women may be somewhat less dramatic. While Wolf and Fligstein's analyses for women yielded unadjusted R^2s ranging from .034 to .108, the present analysis yields adjusted R^2s for women ranging from .089 to .111. However, it should be noted that the higher values in Wolf and Fligstein's ranges apply to regressions containing socioeconomic status as a predictor. The relationship between SES and authority attainment is, to some extent, definitional since jobs involving authority over other workers are jobs that generally rank higher in terms of SES. Unadjusted R^2s range from .052 to .064 for men and from .034 to .084 in Wolf and Fligstein's regressions which exclude SES as a predictor of authority attainment.

The results of Regression 1 suggest that the improvement of the present analysis over that of Wolf and Fligstein does not lie in the inclusion of variables representing the information and influence networks used in getting the job. These variables showed little effect on the authority attainment processes of men and women. None of the separate dummy variables indicating where information about the job was obtained, who helped in getting the job (if anyone), and the extent of influence of those who helped registered any effect significant at the .05 level or higher. There was weak evidence of a positive effect for women of getting help from someone who worked at the firm and had considerable say in the hiring process; but the effect of this variable was significant only at the .10 level, a level lower than that conventionally acceptable for indication of a reliable effect. F-tests showed that all three sets of variables--those measuring the information network used, those measuring who helped, and those measuring the extent of influence of the person helping--also were not significant at the .05 level. Additionally, excluding these variables from the regressions for the subset of workers did not substantially alter the effects of the other predictors included in the analysis; effects of the variables in Regression 1 and Regression 2 were quite similar. Consequently, it appears either that information and influence networks used in obtaining jobs had little impact on authority attainment or that aspects of these networks other than those included in this analysis are the ones that influence authority attainment.

Although there were some differences in the effects of the variables common to both samples--those in Regression 2 and Regression 3--the results in general were quite similar. Since the larger sample was more representative of workers, the discussion below focuses on them.

The regressions in Table 4.3 suggest substantial sex differences in authority attainment processes. Statistical tests for sex differences confirmed this finding. F-tests for the equality of the corresponding male and female regressions in Table 4.3 indicated that the hypothesis of equality can be rejected with 99 percent confidence. Tests for the similarity of individual regression coefficients permitted better identification of the sex differences in the effects of the predictors on authority attainment. Results of these tests for the more representative sample of workers are presented in Tables 4.4 and 4.5. As these tables indicate, the men and women differed significantly in the degree to which the following types of variables affected authority attainment: education, labor force attainment, family situation, and union status of the job. Sex-specific effects of the other types of variables--pre-employer experience, tenure, training, background, and whether occupation is teacher--however, did not differ significantly. We will first review these similarities in the attainment processes of men and women before examining the differences.

Similarities in Authority Attainment Processes of Men and Women

An interesting finding for both men and women concerns the effects of training on authority attainment. As both human capital and institutional theories, would predict, training exerted a strong positive effect on workers' chances of attaining authority. However, consistent with an institutional explanation but not necessarily with human capital theory, training associated with the job had as strong an effect whether it was training yet to be completed or training already completed. In fact, for men the positive effect of an additional year of training yet to be completed was significantly stronger than the positive effect of an additional year of training already completed.[9] Also,

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[9] A test for the equality of sex-specific regression coefficients for the two training variables yielded t-statistics of 2.37 for men and -0.52 for women. This t-test yielded results that are algebraically equivalent to running each sex-specific regression with "years of training completed" (TC) and "years of training needed to become fully qualified for the present job"(TotT) as the set of training measures instead of "years of training completed" (TC) and "years of

Table 4.4 (page 1 of 2)

REGRESSION #4 WITH WHETHER HAS SAY OVER OTHERS' PAY
OR PROMOTION AS DEPENDENT VARIABLE
(For household Heads and wives aged 18-64 and working for someone else in 1976.)

Independent Variable	Coefficient and Standard Error[a] For Variable Listed in Left-Hand Column	 For Variable Listed in Left-Hand Column Times If Male
Years of education	.012** (.004)	.020** (.005)
Years of training completed	.047** (.009)	-.014 (.010)
Years of training not yet completed	.040** (.010)	.010 (.011)
Pre-Employer experience	.0003 (.0026)	-.0041 (.0034)
Pre-Employer experience squared	.0000 (.0001)	.0002 (.0001)
Years with present employer	.0091** (.0029)	.0045 (.0039)
Years with present employer squared	-.0001 (.0001)	-.0001 (.0001)
Fraction of work years fulltime	.172 (.111)	.031 (.194)
Fraction of work years fulltime squared	-.136 (.091)	.065 (.151)
Years not working	-.000 (.001)	-.019** (.005)
Whether fulltime worker	.038* (.019)	.049 (.040)
Hours of work missed due to illness of others in 1975	-.0001 (.0001)	-.0002 (.0003)
Hours of work missed due to own illness in 1975	-.0000 (.0000)	-.0001 (.0000)
Whether placed limits on job hours or location	.013 (.016)	-.041+ (.025)
Whether plans to stop work soon	-.024 (.025)	.030 (.044)

Table 4.4 (page 2 of 2)

Independent Variable	Coefficient and Standard Error[a] For Variable Listed in Left-Hand Column	Coefficient and Standard Error[a] For Variable Listed in Left-Hand Column Times If Male
Whether has disability that limits work	.004 (.031)	.055 (.041)
Whether married	.026 (.026)	.004 (.038)
Whether separated, widowed, or divorced	-.009 (.030)	.096* (.046)
(Whether single)	-	-
Whether has children	.000 (.017)	.044* (.023)
Whether father's occupation ranks high in terms of authority	-.036+ (.020)	.027 (.027)
Whether father's occupation ranks intermediate in terms of authority	.008 (.018)	.024 (.025)
(Whether father's occupation ranks low in terms of authority)	-	-
Whether black	.007 (.024)	.017 (.033)
Whether job covered by a Union Contract	-.035+ (.019)	-.183** (.024)
Whether occupation is teacher	-.208 (.031)	-.062 (.052)
Constant	-.191	-.309** (.031)

R^2 (Adjusted)= .225

Sample Size =4889

**Significant at .01 level.
*Significant at .05 level.
+Significant at .10 level.

[a] All coefficients in this table are the outcome of one single regression, Regression #4. This regression combines observations on both men and women and contains all predictors listed on the left-most column of the table plus sex-interactive terms for all of those predictors; coefficients for the former are listed in the first column of numbers, and coefficients for the latter are listed in the other column of numbers.

Table 4.5

F-VALUES FOR SETS OF INDEPENDENT VARIABLES IN REGRESSIONS PRESENTED IN TABLES 3 AND 4
(For household heads and wives aged 18-64 and working for someone else in 1976.)

Set of Independent Variables	Number of Variables in Set	F-Values		
		For Variables Listed in Left-Hand Column Regression #3[a]		For Variables Listed in Left-Hand Column Times if Male in Regression #4[b]
		Men	Women	Men and Women
Training	2	53.79**	38.20**	1.36
Pre-employer experience	2	7.99**	0.26	2.18
Years with present employer	2	31.07**	17.31**	1.14
Fraction of years worked full-time	2	3.68*	1.78	2.96+
Labor force attachment	9	4.02**	1.63	2.89**
Marital status	2	2.63+	2.67+	3.89*
Father's expected position in authority structure	2	2.94+	4.36*	0.63
Sample size		2723	2166	4889

**Significant at .01 level.
*Significant at .05 level.
+Significant at .10 level.

[a] Coefficients for these sex-specific regressions are presented in the last two columns of Table 4.3.

[b] Coefficients of this regression, which combines observations on both men and women, are presented in Table 4.4; the F-values presented for this regression are F-values for the sex-interactive predictors, with coefficients listed in the last column of Table 4.4.

for both men and women, the positive effect of a year of training yet to be completed was even stronger than the positive effect of an additional year of education.[10]

It is possible that years of training not yet completed reflects uncontrolled positive aspects of a worker's management potential or tastes concerning authority. A similar conjecture was mentioned by Duncan and Hoffman (1978) as justification for adding years of training not completed in wage regressions.[11] If this variable is proxying for such unmeasured qualities, then this finding is not at variance with human capital theory. Whether or not it

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training not yet completed" (TNC). When this is done, the regressions yield the following results for those predictors:

	Men	Women
TC	-.018* (.008)	.006 (.012)
TotT	.051** (.006)	.041** (.008)

Referring back to Regression 3 in Table 4.3, one can see that the following derivation is consistent with these results:

$$S = a + b_1 \text{ TC} + b_2 \text{ TNC}$$

$$\text{TotT} = \text{TC} + \text{TNC}$$

$$\text{TNC} = \text{TotT} - \text{TC}$$

$$S = a + b_1 \text{ TC} + b_2(\text{TotT} - \text{TC})$$

$$= a + (b_1 - b_2)\text{TC} + b_2 \text{ TotT}$$

So b_2 is really the coefficient on TNC and $(b_1 - b_2)$ is the difference between coefficients on TC and TNC.

[10] A test for the equality of sex-specific regression coefficients for education and years of training not yet completed yields t-statistics of 2.68 for men and 3.27 for women.

[11] The conjecture of years of training not yet completed capturing effects of workers skills draws upon an idea mentioned in Duncan and Hoffman (1978) in their analysis of training effects on wages--the idea that training not yet completed could reflect unmeasured worker productivity. They found generally positive wage effects of training not yet completed for both men and women.

captures unmeasured qualifications or tastes, it is at least clearly measuring one aspect of the job--the degree to which training is needed for the job. In combination with the results regarding training completed, the positive effects of training not yet completed indicates that a worker's chances of having authority over other employees was positively and strongly related to training necessary for the job.

Since the effects of training were similar and strong for men and women, differential returns to training associated with the job apparently are not a factor leading to differential chances of attaining authority. Instead, as far as training is concerned, differential placement in jobs requiring substantial training (men averaged higher values on both training measures) would contribute to differences between men and women in their average chances of authority attainment.

Examination of the coefficients on the experience variables suggests that an additional year of tenure had a larger payoff in terms of authority attainment than did an additional year of pre-employer experience. In fact, for men pre-employer experience registered negative effects until 19 years of it were attained, after which the effect became positive; for women effects of pre-employer experience were insignificantly different from zero. Yet the effects of pre-employer experience for men and women were not significantly different. Effects of years with present employer for both men and women were significant and positive throughout the relevant range of possible values for this measure. These results make it difficult to interpret the effects of pre-employer experience on the sex differential in mean levels of authority. However, they do suggest that tenure was more important than pre-employer experience and that differences in levels of tenure contributed more to this sex differential than did differences in returns to tenure: men averaged three and one-half more years of tenure than did women (Table 4.2), but the regression coefficients on the tenure variables for men and women are not significantly different (Table 4.4).

These effects of the experience variables are somewhat different from those in Wolf and Fligstein's (1979) study; however, their sample was one of very young workers only and they did not allow for non-linearities in the effects of their experience measures. The present results are more consistent with Halaby's findings, even though he also did not allow for non-linearities.

The effects of background variables and whether the occupation was primary or secondary teacher also registered no significant sex differences. Of these variables, only the occupation-related variables registered sizeable effects.

Whether or not the individual was black had no independent effect on chances of authority attainment. Father's expected ranking in terms of authority attainment, based on his occupation, had only rather small and non-linear effects. Relative to individuals whose fathers were in low expected ranking, individuals whose fathers were in intermediate rankings were somewhat more likely to attain authority themselves, whereas individuals whose fathers were in high ranking were somewhat less likely to attain authority themselves. Since the effects of this background characteristic were small, however, and sex differences in the characteristic were, likewise small, this characteristic (like race) seemed to contribute little to sex differentials in chances of attaining authority. Sex differences with respect to being a teacher appeared to contribute more substantially to the differential. Being a teacher reduced a worker's chances of having authority over others pay or promotion by over 20 percent, and larger proportions of women than men were teachers (12 percent as opposed to 3 percent). Thus, although men and women were equally less likely to have authority over other employees if they were teachers, the fact that more women than men were teachers reduced women's overall chances of having authority over other workers.

Differences in Authority Attainment Processes of Men and Women

Effects of the following types of variables on authority attainment differed significantly for men and women: education, labor force attachment, family situation, and union status of the job. Looking first at the effects of education, we found that education exerted a large positive effect on a worker's chances of attaining authority; this finding is consistent with both human capital theory and institutional explanations, as well as recent Marxian ideas. We also found, however, that the positive effect was much stronger for men than for women, with the effect being about three times as large for men as for women. These effects of education were similar to those found by both Wolf and Fligstein and Halaby.

Effects of labor force attachment variables tend to lend more support to institutional explanations than to human capital theory. Results concerning the variable "years not working" and the measures of fraction of years fulltime, in particular, and those for all labor force attachment variables combined indicate that weaker labor force attachment had negative effects on authority attainment which were stronger for men than for women. This may reflect a tendency among employers to exclude workers from jobs with authority if they demonstrate the potential for weaker labor force attachment, combined with a tendency to automatically assume that men have strong labor force attachment, unless indications are otherwise, and that women have weak labor force attachment, even if indications are otherwise.

Expectations concerning the effects of family situation and union status of the job were not as clearly associated with a particular theory. However, there were significant sex differences in the effects of these variables. Effects of union status of the job, while large and negative for both sexes, were stronger for men than for women, and a family situation involving children and present or past marriage increased men's chances of authority attainment more than women's.

Interestingly, with respect to marital status, past rather than present marriage had the stronger effect. This may account for the insignificant effects of marital status found by Wolf and Fligstein in their single dummy variable specification indicating whether presently married. The finding concerning effects of presence of children was similar to that of Wolf and Fligstein. With respect to effects of marital status, it may be that while marriage reflects authority potential, present marriage reduces somewhat the ability to respond readily to alterations in work setting, scheduling or efforts that are necessary to attain the authority positions. However, if marital status, along with presence of children, reflects potential work effort why are the effects of these variables significant only for men? Human capital theory would suggest equally strong effects, but in the opposite direction, for women. These findings are more consistent with the notion that employers view family situations as indicators of authority potential only for male workers, an idea which corresponds more closely to institutional explanations of sex differences in the work place.

Accounting for the Overall Sex Difference in Average Levels of Authority Attainment

This section combines the information discussed in the preceding sections to develop an accounting for the observed difference in proportions of men and women who have say over others' pay or promotion. This accounting scheme (Table 4.6) partitions the contributions of the independent variables to the sex difference in the mean value on this dependent variable (male mean-female mean). For each type of variable, the total contribution is divided into two parts: a part due to differences in mean values on the independent variables in the set (column 1) and a part due to differences in regression coefficients (column 2). The former refers to the part of the overall sex difference in the dependent variable which is due to sex differences in average values for the dependent variables evaluated by the effect of those independent variables in the male regression. The latter refers to that part of the overall sex difference in the dependent variable that is due to sex differences in the effects of the independent variables evaluated by the average value of females for those independent variables.[12]

Before delving into a discussion of the major findings of this accounting scheme, we need to note some other aspects of the results. First, sex differences in constant terms impart a very large negative effect on sex differences in chances of attaining authority (Table 4.6). This large negative contribution merely reflects a large and positive contribution of the dichotomous predictors. In wage analyses, the difference in constants, termed "an unexplained shift coefficient" by Blinder (1973), has been attributed to discrimination and combined with the percentage of the difference due to differences in coefficients. (See, for example, Blinder, 1973, and Thurow, 1969.) 1969.) In the present analysis, however, we merely note the contribution

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[12] This accounting procedure differs somewhat from that used by Wolf and Fligstein (1979). The present procedure decomposes differences in independent variables as follows:

$$\bar{Y}_M - \bar{Y}_F = [\Sigma b_{iM}(\bar{X}_{iM} - \bar{X}_{iF})] + [\Sigma \bar{X}_{iF}(b_{iM} - b_{iF})] + [b_{0M} - b_{0F}]$$

> where Y is the mean value on the dependent variable, X_i is the mean value on the ith independent variable, b_i is the coefficient on the ith independent variable, b_0 is the constant term, and the the subscripts M and F refer to males and females, respectively.

The first term in brackets reflects differences in means; the second term in brackets reflects differences in coefficients; the third term in brackets reflects differences in constants. Wolf and Fligstein, on the other hand, in their initial specification decomposed differences in dependent variables as follows:

$$\bar{Y}_M - \bar{Y}_F = [\Sigma b_{iF}(\bar{X}_{iM} - \bar{X}_{iF})] + [(b_{0M} - b_{0F}) + \Sigma \bar{X}_{iF}\,(b_{iM} - b_{iF})]$$
$$+ [\Sigma(b_{iM} - b_{iF})\,(\bar{X}_{iM} - \bar{X}_{iF})]$$

In their procedure, the first term in brackets referred to differences in "composition" or means, the second term in brackets referred to differences in "rates" or coefficients and the third term referred to "interaction" differences. They moved the quantity ($b_{0M}-b_{0F}$) from the rates component to the interaction component when they added job characteristics as independent variables.

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ACCOUNTING FOR SEX DIFFERENCES IN AUTHORITY POSITION
(For household heads and wives aged 18-64 and working for someone else in 1976.)

	Fraction of Authority Gap (in Percent) Accounted for		
	By Differences in Means	By Differences in Coefficients	Total
Skills:			
Education	0.0	168.9	168.9
Years of training completed	17.3	-6.0	11.3
	17.3	169.9	180.2
Experience:			
Pre-employer experience[a]	3.3	-4.8	-1.5
Years with present employer[a]	19.8	11.5	31.3
	23.1	6.7	29.8
Labor Force Attachment:			
Fraction of years full-time[a]	9.2	45.0	54.2
Whether full-time worker	13.0	23.2	36.2
Years not working	69.2	-77.2	-8.0
Other	4.8	-11.2	-6.4
	96.2	-20.2	76.0
Family Situation:			
Marital status	-4.8	15.1	10.3
Presence of children	2.1	14.6	16.7
	-2.7	29.7	27.0

Table 4.6 (page 2 of 2)

	Fraction of Authority Gap (in Percent) Accounted for		
	By Differences in Means	By Differences in Coefficients	Total
Background:			
Father's expected authority position	0.0	11.9	11.9
Race	0.0	1.3	1.3
	0.0	13.2	13.2
Job Characteristics:			
Years of training not yet completed	10.5	1.4	11.9
Whether job covered by union contract	-20.9	-22.4	-43.3
Whether occupation is teacher	10.3	-3.5	6.8
	-0.1	-24.5	-24.6
Constant			-201.7

[a] Includes squared as well as unsquared term.

of the difference in constants and confine our discussion to comparisons of the relative magnitudes of the roles of the independent variables in accounting for authority differences between men and women. We also note, however, that the results include inconsistencies between Table 4.6 results and significance tests for differences in coefficients pertaining to several variables. Despite showing no significant sex difference in the effects of experience and background variables, Table 4.6 shows small but non-trivial contributions from differences in coefficients for these variables.

Turning to other findings, we see that education, primarily, and full-time vs. part-time labor force attachment, secondarily, played major roles in contributing to the sex difference in the proportions of workers with say over others' pay or promotion (Table 4.6). Differences in returns to education, and not differences in mean levels of education, contributed substantially to the sizable role of this variable in sex differences in authority. The accounting procedure also indicates that differences in the returns to full-time labor market attachment contributed more to sex differences in authority than did differences in the extent of full-time labor market attachment, although the latter also made a contribution. Recall, however, that sex differences in the separate effects of the variables comprising full-time labor market attachment were not quite significant at conventional levels. It is clear, though, that if the effects of education were the same for women as for men, women's chances of being in positions of authority would be considerably enhanced. Other factors that contributed to men being more likely to have authority than women were: training needed for the job (combining years of training completed with years of training not yet completed), family situation, and occupation being a teacher. The overall contributions of training needed for the job and family situation were of similar magnitude.

For training, differences in levels of these characteristics rather than differences in their effects were the primary factors affecting to sex differences in authority. Women were less likely to have authority over others because they were less likely to be in jobs that required substantial training; contributions of training completed and training not yet completed were quite comparable. Since sex differences in mean levels of these characteristics were not huge, however, effects of the training variables on the sex difference in authority were rather small.

For family situation, the sex-specific effects of the variables mattered more. While somewhat larger proportions of working men than working women were in family situations involving children and/or present or past marriage, it was primarily the greater returns to these situations for men that increased their chances of authority attainment relative to those of women.

Being a teacher accounted for only a relatively small, albeit positive amount, of the sex difference in the proportions of workers with authority over others. As with experience and training, the major contribution of the teaching occupation was due to differences in the proportions of men and women who were teachers rather than differences in the effect of being a teacher; women were somewhat less likely to hold positions of authority because somewhat larger proportions of them were teachers.

A job characteristic with a more sizable contribution to the sex difference in authority was union status of the job. The accounting process indicated that differences both in the proportions of men and women in union jobs and in the sex-specific effects of union status of the job affected the sex difference in authority. However, the negative signs on the contributions of this variable suggest that if as large a proportion of women as men were in union jobs and if being in a union job had as large an effect for women as for men, then women would be even less likely to be in positions of authority.

The overall contributions of the remaining factors, years not working and other indicators of restricted labor force attachment, were very small. They were small for an interesting reason, though. As the means in Table 4.2 indicate, women did tend to have weaker labor force attachment; however, a weak labor force attachment had a stronger negative effect on attaining authority for men than for women. The latter force tended to somewhat more than counteract the former, resulting in a small negative overall contribution of these variables.

SUMMARY AND CONCLUSIONS

The present analysis focuses on attaining authority at work and examines differences in the attainment processes of men and women using samples of working, non-self-employed household heads and wives. This analysis yields some results similar to work on socioeconomic status attainment. These similarities pertain to the effects of education and background factors: education showed a much stronger effect both on attaining authority and on attaining socio-economic status attainment than did father's corresponding level of attainment. The effect of education was large and positive for both men and women. However, although work on socioeconomic status attainment has indicated substantial similarities in the mean levels and attainment processes of men and women, there are clear sex differences in authority attainment. Men are much more likely than women to have authority at work. The Panel Study data show that while one-quarter of working, non-self-employed men have supervisory responsibilities that involve say over other workers' pay or promotion, only one-tenth of working non-self-employed women are in similar positions of authority. It is this aspect of authority that the present analysis concentrates on.

Not only do men and women differ with respect to their average chances of attaining such authority, they also differ with respect to the roles various factors play in gaining authority. The present analysis of these non-self-employed workers, like that of Wolf and Fligstein and Halaby, shows that education has a much stronger effect on authority attainment among men than among women. Although men and women averaged similar levels of education, each year of education has about three times as strong a positive effect on men's chances of having say over others' pay or promotion. This difference in the payoff to education accounts for a substantial proportion of the sex difference in authority attainment.

Effects on authority attainment of labor force attachment and family situation are also stronger for men than for women. Men are penalized more severely than women for weak labor force attachment, but unlike women, their chances of gaining authority may be improved by family ties. Men with children and men no longer married are somewhat more likely to have say over other workers' pay or promotion. This difference in effects of family situation (which is consistent with Wolf and Fligstein's findings) makes a small positive contribution to the sex difference in chances of gaining authority. Differences in effects of labor force attachment have a somewhat stronger effect; if women received the same rewards as men for a past and current record of full-time labor force commitment they would be far more likely to be in positions of higher authority. Additionally, since full-time labor force commitment has positive effects on authority attainment for women as well as men, if larger proportions of women had past and current records of full-time labor force attachment, women's average chances of attaining authority would be somewhat closer to men's.

Training needed to become fully qualified for the job, both completed and not yet completed, and of experience, both years working prior to employment with the present employer and tenure with the present employer, show similar effects for men and women. For both, tenure has a stronger positive effect than years of work prior to employment with the present employer, and training needed to become fully qualified for the job has a strong positive effect, of similar magnitude regardless of whether it is years of training already completed or years of training yet to be completed. Interestingly, for men as well as women a year of training yet to be completed has a stronger positive effect on authority attainment than does a year of education. These strong effects of training contribute to the sex difference in mean authority level, not because of sex differences in effects of training on authority attainment, but because of sex differences in mean levels of training. Women average less training, both completed and yet to be completed, than do men.

With more specific measures of type of job, we find that, for both men and women, union and teaching jobs are less likely than non-union and non-teaching jobs to involve high positions of authority. While women are less likely than men to be in union jobs, they are more likely to be teachers.

These results suggest that for both men and women authority attainment depends heavily on the type of job that a worker gets. Yet, although the results tend to be more consistent with institutional explanations of sex segregation, inadequate controls for workers' tastes and work effort may mask the possible applicability of human capital theory. Jobs that require substantial amounts of training and education apparently have promotional ladders leading to positions of authority. These jobs would tend to correspond to primary sector jobs. The finding of equal effects of years of training completed and not yet completed are consistent with the institutional explanation that the job held, rather than the qualifications of the worker, determine the worker's rewards, whether the worker is male or female. However, years of training not yet completed could reflect either a worker's tastes or the ability to acquire further skills, making this finding consistent with human capital theory.

The finding that men, more than women, are likely to get these higher authority jobs if they demonstrate higher education credentials, strong labor force attachment, and family formation is also at variance with human capital theory. However, human capital proponents could attribute this finding to sex differences in work effort or tastes concerning the desire for responsibility. Since the present analysis has no direct controls for these factors, we cannot clearly rule out such a role for these variables.

While we find that authority attainment depends heavily on the type of job a worker gets, we also find that our measures of information and influence networks used in getting jobs have little effect on attaining authority. The sex-specific analysis involving a subset of the younger, working, non-self-employed adults indicates that among both the men and the women information and influence networks used in getting jobs have little effect on the likelihood of getting into a position with say over others' pay or promotion. However, access to information and influence networks may differ in ways not tapped by the present analysis. It may be that women tend to be restricted to information and influence networks associated with jobs involving lesser authority, whereas men may have greater access to information and influence networks associated with higher authority jobs.

Thus, although this analysis adds to understanding of the processes of attaining authority and how those processes differ for men and women, it leaves much room for further additions to our understanding. We need comprehensive measures of workers' tastes and work effort, employers' hiring and promotional practices, and access to information and influence networks associated with particular types of jobs in order to more fully understand the processes of authority attainment.

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Chapter 5

THE EFFECTS OF COMMUNITY AND FAMILY BACKGROUND ON THE EDUCATION AND EARNINGS OF BLACK AND WHITE MEN

Linda Datcher

INTRODUCTION

In recent years, considerable attention has been devoted to understanding the causes of earnings differentials between blacks and whites. Conventional economic analysis of the issue has been divided into supply and demand factors. Demand side arguments point to differential returns to blacks and whites with the same level of market-valued characteristics as a principal cause of the lower earnings of blacks. These differential returns have been attributed to (1) discrimination on the part of white employers and workers who harbor a distaste for association with blacks and (2) statistical discrimination of white employers due to risk aversion and greater uncertainty about the correlation between observed characteristics of blacks and actual on-the-job performance. Supply side arguments, on the other hand, focus on the quality and quantity of the market-valued characteristics of black compared to white workers. They conclude that, even in the absence of discrimination, blacks would earn less than whites because they have acquired lower levels of these characteristics.

Although this dichotomization is obviously a necessary and useful step in understanding the causes of earnings differentials between blacks and whites, it fails to place sufficient emphasis on the process by which market-valued characteristics are actually obtained. As a result, it does not illuminate the intertemporal consequences of racial discrimination stemming from the effect of parents' economic status on their offsprings' acquisition of market-valued characteristics. Furthermore, since it focuses primarily on discrimination in terms of what one individual does to another, it obscures many of the negative externalities affecting the acquisition process that result from being part of a discriminated-against group.

Remedying these deficiencies is important to understanding and eliminating earnings differentials between black and white workers for a number of reasons. First, as numerous analysts (Smith and Welch 1977, 1978; Weiss and Williamson

1972; and others) have indicated, differences between blacks and whites in the level of market-valued characteristics account for much of the earnings gap. According to Smith and Welch (1977), lower levels of schooling for blacks alone accounted for the largest part of the black-white earnings differential in 1970. Second, other analysts (e.g., Bowles 1972) assert that failure to take into account social background factors leads to over-estimates of the effect of schooling on earnings. They argue that schooling serves as a proxy for socioeconomic background and that its effect independent of the background factors is limited. Third, understanding the process by which market-valued characteristics are acquired has important public policy implications. Loury (1976) has developed a theory of racial income differentials which focuses on this process. A primary motivation behind this theory was to examine whether a laissez-faire policy of equal opportunity is sufficient to lead to the eventual elimination of racial economic inequality or whether more affirmative policies intervening on behalf of blacks are required. His findings focus on whether the process by which parental economic status influences the skill acquisition of offspring functions less effectively for blacks than for whites. He shows that, if this happens because of the negative externalities of being part of a historically discriminated-against group, then the ability of a laissez faire policy of equal opportunity to eliminate racial economic inequality is seriously weakened.

This chapter addresses some of the problems suggested above by examining a recursive model of the effect of socioeconomic background on education and earnings of black and white men aged 23-32. It differs from other efforts in this area in three primary ways. First and perhaps most important, it includes data reflecting not only the socioeconomic status of an individual's parents, but also characteristics of the individual's neighborhood of origin. To the extent that community characteristics either magnify or offset the influence of parental socioeconomic status, they become important in understanding racial income differentials. Second, the data include income information for the family of origin. Third, since the data are derived from longitudinal records, the respondent's recall is not such a critical factor.

The chapter is divided into three sections. The first section presents the theoretical foundation behind the analysis and specifies testable hypotheses drawn from the theory. The second describes the data and the empirical model used to test the hypotheses, and the third presents the empirical results and conclusions.

THEORETICAL FRAMEWORK

The theory behind examining the effect of background on education and earnings comes from earlier work by Loury (1976), Becker (1965, 1975), and Ben-Porath (1967). The basic model begins with parents' decisions about how much of their resources to spend on activities and purchases that raise their children's level of human capital or other market-valued characteristics. Using the household production theory framework (Becker, 1965), families are assumed to combine time and market goods to produce the commodities that directly enter their utility functions. They are subject to the constraint that full income (the income that could be obtained by devoting all the time and other resources of a household to earning income) equals income spent directly on market goods and income foregone to obtain additional utility. In a two-commodity world, full income $Y = p_1a_1Z_1+p_2a_2Z_2+L(Z_1,Z_2)$ where the Z_i are the commodities, the p_i are the price of the goods purchased on the market, the a_i are the inputs of market goods per unit of Z_i, and L is the total income foregone to obtain additional utility. Families make decisions about on how much Z_1 and Z_2 to obtain given their preferences, their full income, the direct component of the total marginal price, p_ia_i, (the expenditure on market goods required to obtain an additional unit of Z_i), and the indirect component of the total marginal price, $\partial L / \partial Z_i$ (the earnings foregone to acquire another unit of Z_i).

In Figure 5.1, parents A and B face the same total prices and have the same initial level of full income, Y_1. Parents A spend R_3 on investing in their children, while parents B spend, R_1, a smaller amount, because their taste for other goods is higher. As the incomes of both sets of parents increase to Y_2, they spend more, R_4 and R_2, respectively. In Figure 5.2, parents A and B have the same level of income Y and the same preferences. However, parents B must pay more for investing in their children. As a result, the amount of investment parents B actually undertake, R_5, is lower than that of parents A, R_6.

Sons decide on the amount of additional investment to undertake given the level made by their parents, their ability, and the return from the additional investment. The total amount of investment (C_1 in Figure 5.3) depends on the location of the intersection of the expected marginal revenue (MR) and marginal cost (MC) curves associated with adding one more unit of a given market-valued characteristic. Marginal revenue is the discounted flow of future increases in earnings due to the additional unit, while marginal costs consist of all corresponding expenses borne by the son. These expenses include foregone earnings, financing costs, tuition fees, etc. Higher parents' expenditure and

Figure 5.1

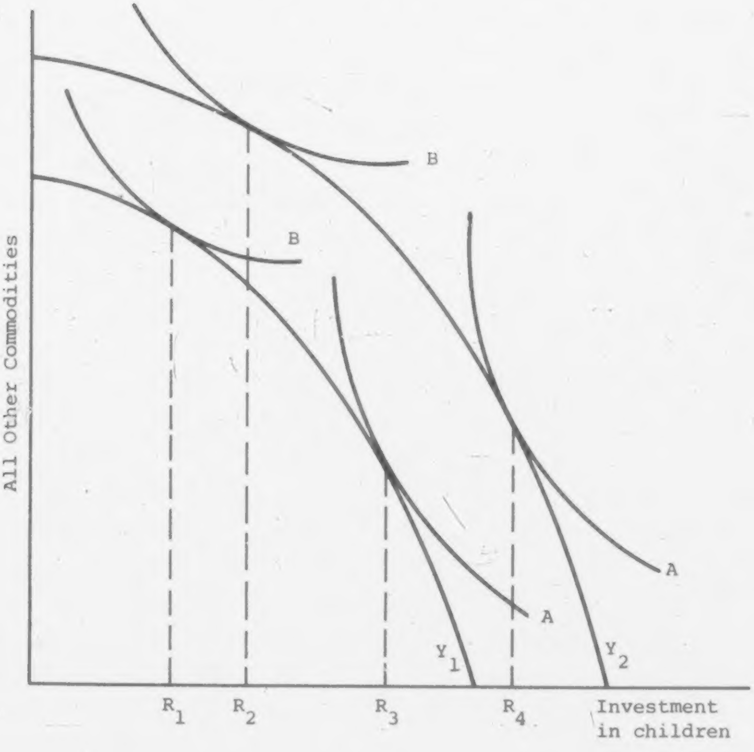
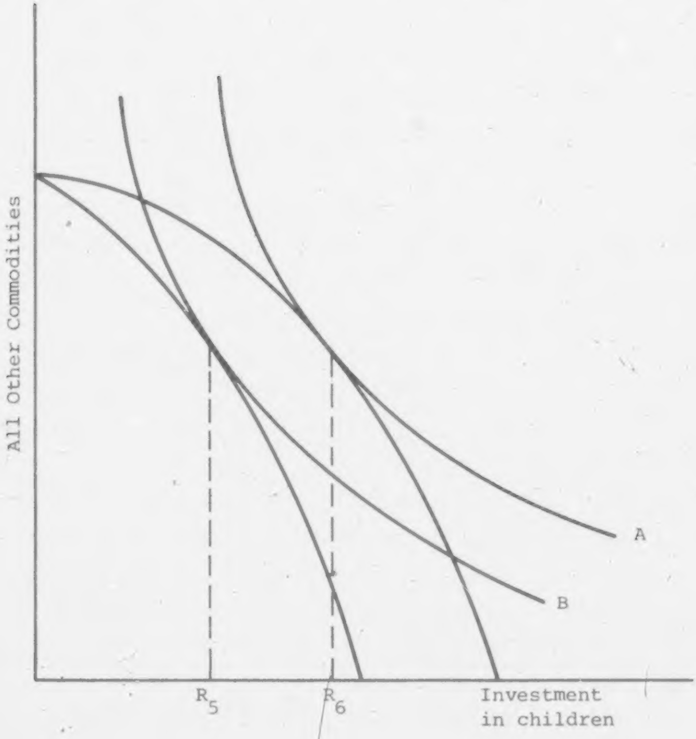

All Other Commodities
B
B
A
A
Y_1
Y_2
R_1
R_2
R_3
R_4
Investment in children

Figure 5.2


All Other Commodities
A
B
R_5
R_6
Investment in children

higher son's ability lower the amount of the son's own resources needed to acquire a given level of market-valued characteristics and, thus, lower the son's marginal cost curve (e.g., to MC′ in Figure 5.3). Holding constant the expected marginal revenue curve (MR), the level of acquisition rises to C_2. Similarly, holding constant the marginal cost curve (MC), a jump in the marginal revenue curve (MR′) raises the acquisition to C_3.

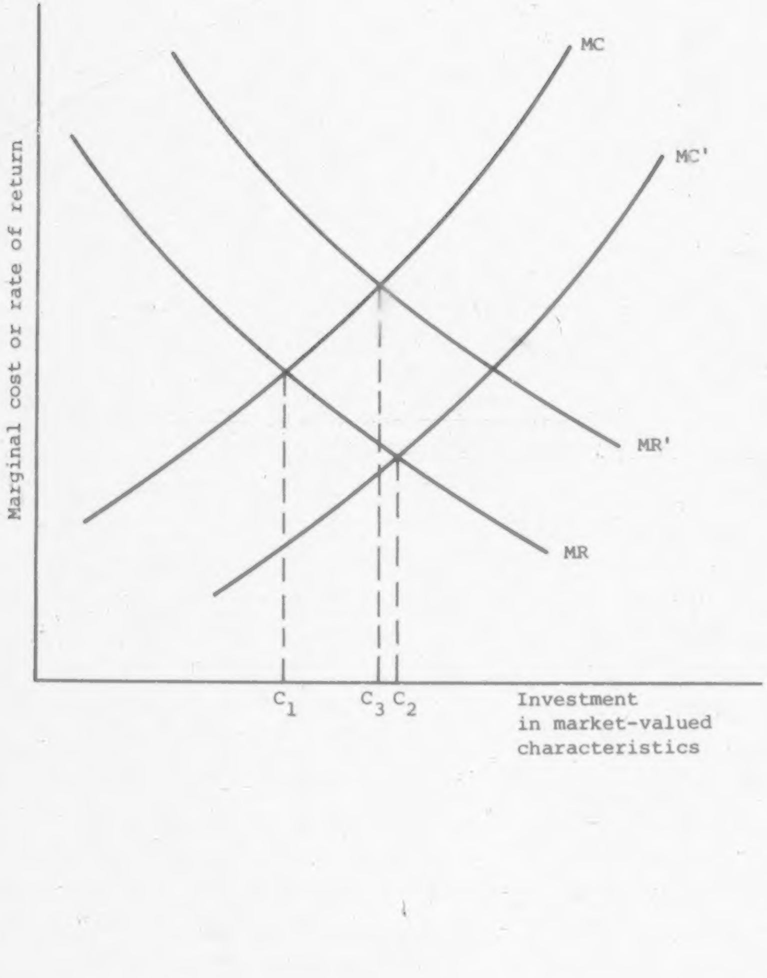
This analysis indicates that three of the basic factors that determine the son's actual level of investment include (1) parents' investment, which depends on their preferences, full income, and direct and indirect prices; (2) son's ability, and (3) son's expected rate of return on investment.

If data were available on all these factors and the total level of investment in the son, then the theory implies that an empirical model would yield relatively straight-forward findings. Higher son's ability, larger rates of return on investment, and higher levels of parental parameters which increase the amount parents invest would raise the total level of investment in the son. This would, in turn, increase the son's earnings. Lower son's ability, lower rates of return, and higher levels of parental parameters which lower the amount parents invest would have the opposite effect.

The empirical model used here differs from this ideal model in two important ways. First, precise measures of the three basic factors mentioned above were not available for this analysis. Second, although the individuals in the sample had already completed their formal schooling, they may still have been making large investments in post-school training. The first problem was lessened by using a number of proxies and indirect measures for the three basic factors. The second, on the other hand, resulted in different expectations for the effect of background on education, the completed investment, and the effect of background on the acquisition of other market-valued characteristics as indicated through its effect on earnings. The remainder of this section will first point out the effects of continuing investment through on-the-job training and then discuss the implications of using the proxies and indirect measures.

For individuals who were continuing their training through on-the-job learning and experience, the effect of background on current earnings would understate the importance of background factors in determining earnings capacity. Assuming that training is costly in terms of earnings foregone, Figure 5.4 shows representative age/earnings profiles for individuals who undertake different amounts of post-school training. Workers in groups A and W are identical in every way except that, because W's have more favorable backgrounds, they acquire

Figure 5.3


Marginal cost or rate of return
MC
MC'
MR'
MR
C_1
C_3
C_2
Investment
in market-valued
characteristics

on-the-job training after schooling. An examination of the effect of background on earnings for A's and W's between the ages u and v shows that favorable backgrounds would have a negative effect on earnings even though this would not be true over the entire age range. This would be less of a problem for workers in group B because they acquire less training and have a less steep age/earnings profile than group W workers. According to findings of various analysts (Lazear, 1979, and Duncan and Hoffman, 1978), the B and W profiles may be representative of blacks and whites, respectively, who acquire post-school training. This suggests that looking at the effect of background on earnings for young men would underestimate the ultimate importance of background and that the bias will be more severe for whites than blacks.

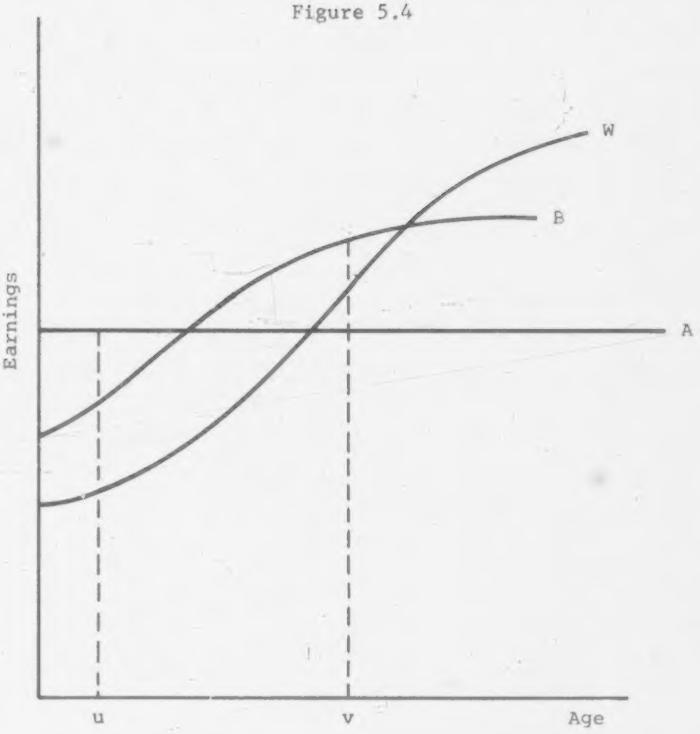
As mentioned earlier, since precise measures of son's ability, son's expected rate of return on investment, and parental parameters which affect the level of investment in the son (prices, income, and preference) were not available for the analysis, a number of proxies were used. These included:

(1) Father's years of education
(2) Mother's years of education
(3) Total income of the son's family of origin
(4) Region of origin
(5) Number of siblings
(6) Size of place of origin
(7) Age of head of son's family
(8) Average family income in neighborhood of origin and
(9) Percentage of whites in neighborhood of origin

The principal expected effects of these variables on son's education and earnings can be derived from their relationships with the three basic factors listed above. For example, higher father's years of education, lower number of siblings, and higher mother's years of education would be positively correlated with parental preferences for higher investment in a given child. Leibowitz (1974) has shown that the quantity of time devoted to children is positively related to parents' education. Furthermore, the parental education variables are suggestive of the value parents place on such activities for their children, while the number of siblings indicates the extent of competing claims for a given amount of family resources.

The parental education variables, in addition to total family income and the age of the head, are also proxies for full family income. Higher parental human capital in the form of more education raises the level of earnings that the family can expect to achieve. Holding constant parents' education, higher family income measures other variables that also raise full family income. These include parents' ability and parents' out-of-school training and work experience,

Figure 5.4



as well as the earnings enhancing characteristics of the parents' occupations and industries. Holding parents' education and current income constant, there is evidence that income over the son's early life may also be affected by the age of the head. Table 5.1 shows that, for both black and white men, the earnings profile of men in older cohorts is, in general, less steep than that of younger men with the same level of education. This suggests that sons with older parents who had the same level of family income as sons with younger parents enjoyed relatively higher parental income in their earlier years. Since the difference between the older and younger cohorts is less prominent for blacks than whites, the effect of the age of the head on education and earnings for black sons would also be smaller than the effect for white sons.

Table 5.1

DIFFERENCES IN INCREASE IN AVERAGE EARNINGS
FROM 1959 TO 1969 BY AGE IN 1959, EDUCATION, AND RACE

	Change for 35-44 Year Olds- Change for 45-54 Year Olds	
	Black	White
Total	714	1549
8 years education	534	855
11 years education	362	507
12 years education	565	1037
16+ years education	1355	2875

Sources:

U.S. Bureau of the Census, U.S. Census of Population: 1970, Subject Reports, Final Report PC(2)-5B, Educational attainment, Table 7.

U.S. Bureau of the Census, U.S. Census of Population: 1960, Subject Reports, Occupation by Earnings and Education, Final Report PC(2)-7B, Table 1.

The prices paid by parents to invest an additional unit in sons include both the expenditure on market goods as well as the earnings foregone. To a large extent, these prices are influenced by the type and "quality" of the community in which the son matures. For example, differences in educational and occupational achievement between urban and rural youth have been noted in the past by sociologists (Sewell, Haller, and Strauss, 1957; Sewell, 1964). They attribute this finding to community differences in tastes and knowledge of available opportunities. Regional differences in the cost of acquiring human capital may also be important, especially for blacks in the South.

In addition to these locational and geographic factors, the "quality" of the community also affects the direct and indirect prices paid by parents (Loury, 1976, examined this issue in more detail). To illustrate the effect, imagine two families who are identical with respect to composition, financial resources, preferences, and abilities of their members. The community in which the first family lives has a higher average level of income and educational attainment than that of the second family. In such a case, a son in the first family would benefit much more from his community environment than a son from the second family. He may attend higher quality elementary and secondary schools. The peers with which he interacts would probably have higher educational and occupational aspirations. More information about higher quality jobs and careers would be available from neighborhood people. Some people may even be directly able to help him acquire higher level jobs through recommendations, information about job openings, etc. Therefore, while the first son would have easy access to many market-valued characteristics at low cost, the parents of the second son would have to pay more to make such information and opportunities available to him because of the negative externalities associated with living in a lower income community. This implies that, in the empirical model used in this analysis, the average level of family income in the community of origin would compensate in large part for the omission of data on prices not taken into account by geographic location and size of place of origin.

Some differences between blacks and whites in the expenditure of time and money required to invest an additional unit in children may remain, however, because of current and past racial discrimination. Black parents may not have access to the same information and channels of influence as white parents with the same education and income. Predominantly black communities with high levels of average income may be similarly handicapped relative to a white neighborhood. If this were true, then blacks in predominantly white neighborhoods would have higher education and earnings, other things being equal, than those from black neighborhoods. Furthermore, an increase in black parental education, family income, or community income would have a smaller effect on black sons' education and earnings than a corresponding change for white sons because of differences in the expenditure required to obtain an additional unit of investment. (Note that attributing observed differences in returns to investment for a given level of parents' education, family income, or community income to differences in price assumes that black preferences for investment are not lower than white preferences, other things equal.)

The effects of differences in son's ability and expected rates of return on investment on son's education and earnings are not directly taken into account by the available background measures. The omission of son's ability in the earnings regression, holding constant son's market-valued characteristics, understates the importance of background on earnings and overstates the significance of the market-valued characteristics. (See Becker, 1975, pp. 118-120, for a more thorough discussion of this issue). Sons with more favorable backgrounds would invest the same as those with less favorable backgrounds only if the former were to have lower abilities. This indicates that, holding constant market-valued characteristics, better backgrounds are negatively correlated with ability. Similarly, holding constant background characteristics, the levels of acquired market-valued characteristics are positively correlated with ability.

Even if corrections are made for this problem, the omission of son's ability implies that the effect of parents' education and income on son's education and earnings overestimates the effects of non-inherited parental resources. This follows directly from noting that ability is, at least partly, genetically determined, that parents' education is positively correlated with parents' ability, and that, holding constant parents' education, family income is also positively correlated with parents' ability. The omission of expected rates of return on investment is probably most important for black/white comparisons. To the extent that black sons base their own earnings prospects on older blacks who have been subject to substantial labor market discrimination, their expected rate of return would be lower, and the amount they would invest with a given set of background characteristics would be lower than that of a white son with the same background. Such differences, however, are likely to be much less important than in the past, given that black sons may rely more on perceptions of current rates of return which are much closer to those of white sons (Smith and Welch, 1978).

Summarizing the theoretical implications of the effect of background on son's education and earnings leads to a number of conclusions. First, since the amounts parents invest in their children are raised by increases in parents' education, family income, community income, urban origin, percentage of whites in neighborhood of origin, and age of head, each of these variables should have a positive effect on son's education and earnings. Second, for similar reasons, farm origin, southern origin, and number of siblings should have a negative effect. Third, the effect of the age of the head should be larger for whites than blacks because of smaller cohort differences in age/earnings profiles for blacks. Fourth, the effects of black parents' education, family income, and community income should reflect the higher prices faced by black parents and, therefore, should have smaller effects on black's education and earnings than the corresponding variables have on white education and earnings. Fifth, the coefficients of background variables in an earning regression would understate the effect of background on earnings for the entire sample because many would still be acquiring training. The oldest members of the sample would be closer to completion of their training, so that their background effects should be higher. Sixth, most or all of the predicted larger effect of white backgrounds on earnings may be offset because white sons would be more likely to be in training.

DESCRIPTION OF THE DATA AND THE EMPIRICAL MODEL

The sample used to test these hypotheses was taken from the eleventh wave of the Panel Study of Income Dynamics. It consists of employed male heads of households aged 23-32 who lived with their parents in one of 188 SMSAs in 1968. One of the parents of these sons (i.e., the head of the household) was interviewed as part of the original sample in 1968. These sons subsequently left home to form their own households.

The principal advantage of using this subset of sons is that, since the parents were (and, in most cases, still are) respondents or spouses of respondents, a wide variety of information about each son's family of origin is directly available from the current and past interviews with the son's mother or father. As mentioned earlier, most other studies have been forced to rely on retrospective reports by the son on a very limited range of data concerning his family background. Men under 23 were not included in this sample because a large fraction of them, particularly those with the highest ability and those with the most favorable backgrounds, would still be in school. Men over 32 were excluded because access to most of the family background data hinges on both the parents and the son being part of the sample. This is true only for sons who lived with their parents in 1968. Men over 32 would have been older than 22 in 1968. Therefore, the inclusion of only those living at home would bias the sample toward those with families more willing and/or able to continue to support their sons.[1] The sample was restricted to sons living in a SMSA in 1968 because community data was available only for this group. The source of the community data was summary records from the 1970 Census Fifth Count for Zip Codes. These records contain information about various socioeconomic characteristics of SMSAs by five-digit zip code number. Although the five-digit zip code area may not correspond exactly to various conceptions of what encompasses a neighborhood, it provides much finer indicators of local characteristics than city, county, or state measures. Because of the geographic restriction, however, the sample is not necessarily representative of sons living outside of SMSAs.

The recursive model of the effect of socioeconomic background on education and earnings was derived from the Duncan et al. (1972) life-cycle model. It postulates a temporal sequence of events in an individual's life that determine earnings. In particular, it states that (1) the life cycle of an individual can be broken into three stages--background, schooling, and job, and (2) the earnings of an individual are determined through the following recursive system--(a) schooling depends on background, (b) occupational achievement depends on schooling and family background, and (c) earnings depend on occupational achievement, schooling, and background. The specific model used here excludes the occupational achievement stage because a detailed occupational classification was not available for the Panel Study sons and using broad occupational groups added little to the analysis. A path analysis diagram of the model is given in Figure 5.5. Family and community background have direct effects on both education and earnings and indirect effects on earnings through education. The direct effect of the background variables on earnings comes, in large part, from their impact on unmeasured market-valued characteristics that, in turn, determine earnings.

The community background measures that were available included:

(1) Percentage of employed persons, 16 years or older, in professional, technical, or managerial category

(2) Percentage of employed persons, 16 years or older in service or laborers category

(3) Percentage of men, 16 years or older, who were unemployed

(4) Percentage of persons, 25 years or older, with less than 12 years education

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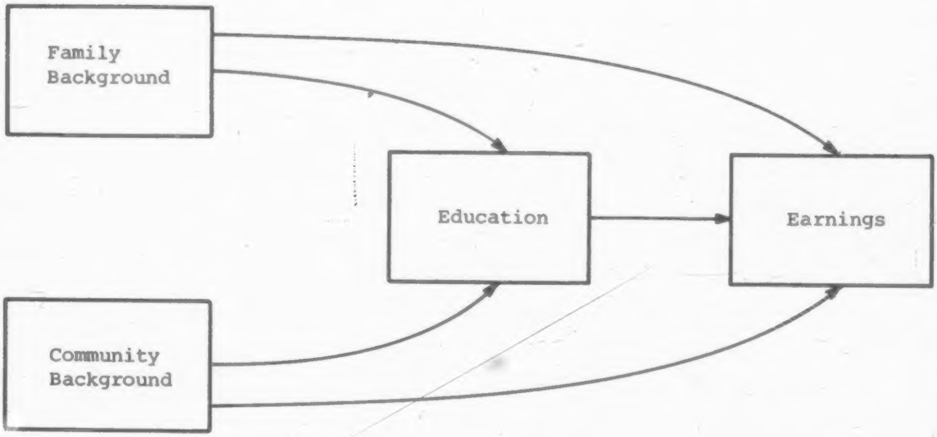
[1] Including sons aged 29-32 results in a similar selectivity bias. This is corrected using the methods described by Heckman (1979).

Figure 5.5


Family Background
Education
Earnings
Community Background

(5) Average family income of total population
(6) Average family income of black population
(7) Percentage of persons, 25-44 years old, with less than 12 years education
(8) White percentage of population

The average family income measure and white percentage of population were the only ones reported here because the others showed no independent effect on son's education and earnings. The specific family background measures that were also used included:

(1) 1968 father's report of his own education (or son's report of father's education for female-headed families)
(2) 1968 head of household's report of mother's education
(3) Total 1968 income of family of origin
(4) Son's report of size of place of origin
(5) Son's report of region of origin
(6) 1968 number of siblings
(7) 1968 age of head of family of origin

Although few other studies include family incomes measures, most control for father's occupational status. As in the case of son's occupation, the broad occupational groups available from the Panel Study provide little additional information and so regression results including father's occupation are not reported here.

EMPIRICAL RESULTS

Table 5.2 presents the results of the education regression analysis. Two equations for blacks are listed, with the second one including the percentage white in the neighborhood of origin. Looking at the non-income variables in the regressions shows that, as predicted, the coefficients of years of father's education, age of head, and city origin were positive, and the coefficient of number of siblings was negative. For both races, the size of the coefficient of father's education was substantially larger than the insignificant though positive coefficient of mother's education. This may reflect the greater importance of father's education in determining both family income and parental expectations for male children. The absolute sizes of the coefficients for age of head and number of siblings for blacks were smaller than for whites. This finding is consistent with the existence of differential costs to blacks of investing in children and flatter age/earnings profiles mentioned earlier.

Table 5.2 (page 1 of 2)

RESULTS FOR YEARS OF EDUCATION REGRESSIONS
(For employed male household heads aged 23-32.)

	Black	Black	White
Father's education	.1998 (.0456) [.4131]	.1859 (.0456) [.3842]	.1947 (.0517) [.2753]
Mother's education	.0297 (.0521) [.0598]	.0581 (.0534) [.1171]	-.0035 (.0620) [-.0040]
Rural origin	1.8078 (.5031) [.2801]	1.7139 (.4999) [.2656]	-.5549 (.4055) [-.0694]
City origin	.6667 (.2700) [.2036]	.7396 (.2695) [.2258]	.5793 (.2355) [.1199]
Southern origin	.2383 (.2614) [.0731]	.1966 (.2594) [.0603]	-.0326 (.3312) [-.0050]
Number of siblings	.0011 (.0474) [.0017]	-.0165 (.0476) [-.0258]	-.2712 (.0548) [-.2365]
Age of 1968 head of household	.0567 (.0136) [.3030]	.0538 (.0135) [.2878]	.0765 (.0157) [.2341]
1968 family income <8000	.0712 (.0718) [.0921]	.0641 (.0711) [.0830]	
1968 family income ≥8000	-.0836 (.0612) [-.1446]	-.0824 (.0605) [-.1425]	
1968 family income <5000			-.2256 (.2702) [-.0445]
5000 ≤ 1968 family income <11,000			.1370 (.0755) [.1216]

Table 5.2 (page 2 of 2)

	Black	Black	White
11,000 $\leq$ 1968 family income <14,000			-.3424 (.1512) [-.1933]
14,000 $\leq$ 1968 family income <17,000			.4891 (.1854) [.2213]
1968 family income $\geq$ 17,000			-.0841 (.0613) [-.0836]
1969 community income <8,000	-.2513 (.2156) [-.1014]	-.3516 (.2189) [-.1418]	.6347 (.3688) [.1019]
1969 community income $\geq$ 8,000	.2509 (.0856) [.2596]	.1716 (.0931) [.1776]	
8,000 $\leq$ 1969 community income <11,000			-.2099 (.1620) [-.0846]
11,000 $\leq$ 1969 community income <17,000			.1901 (.0795) [.1444]
1969 community income $\geq$ 17,000			.0682 (.0851) [.0393]
1970 percentage white in community		.0102 (.0050) [.1758]	
Intercept	8.5806	9.0802	4.6329
R^2	.3907	.4492	.3662
N	158	158	329

Standard errors are in parentheses.
Standardized coefficients are in brackets.

Rural and southern origin had a negative but insignificant impact on education of whites. While the same was true for southern origin for blacks, rural origin had a large, positive and significant effect. Since the sample consisted only of individuals who lived in an SMSA in 1968, this observation indicates that individuals who spent much of their early lives in rural areas, but whose families had migrated to an SMSA by 1968, acquired more schooling than those who lived in other areas for most of their lives. This suggests that such migration may be correlated with other characteristics conducive to educational success and that those who have recently migrated represent a select group within the overall black urban population.

Spline variables were used to examine the effects of family and community income on son's education since they would uncover non-linearities in the effects of income. If black family income spline breaks were at $8,000 and $11,000, families with income of $7,000 would be recorded with family income <8000 =7, 8000≤ family income <11000 = 0, and family income ≥11000 = 0, and families with income of $15,000 would be recorded as family income <8000=8, 8000≤ family income <11000=3, and family income ≥11000=15-11=4. Thus, at each level values were assigned according to how many thousands of dollars the family had within a particular income range. The placement of the breakpoints was determined through regression analysis that included finer intervals and through F-tests to determine which interval coefficients could be constrained to be the same. The coefficient of family income <8000 indicates the change in son's education per thousand dollar change in family income from $0 to $7,999. The coefficient of 8000≤ family income <11000 shows the effect of each additional $1,000 in the $8,000 - $10,999 range. The other coefficients can be interpreted in the same manner.

While family income had no significant impact on years of education for black sons, the white family income coefficients revealed an interesting pattern. Each increment of income less than $5000 had no effect on sons' education, additions in the $5000 to $11,000 range raise it, additions in the $11,000 to $14,000 range lowered it, and increments above $14,000 raised it again. The reason for this pattern can be determined by looking separately at the effect of family income on years of pre-college education and years of college education. Table 5.3 shows the effects of the family income variable holding the other measures of family background constant. It indicates that, the negative effect of education in the middle ranges stemmed principally from a reduction in the years of college. This suggests that individuals who came from families in this range may have had avenues other than college education open to them for finding good jobs.

Table 5.3

REGRESSION RESULTS FOR YEARS OF
PRIMARY/SECONDARY SCHOOLING
AND YEARS OF COLLEGE
(For employed white male heads of households aged 23-32.)

	Years of Primary/ Secondary Schooling	Years of College
1968 Family income <5000	.0605 (.0987)	-.2843 (.2413)
5000≤ 1968 family income <11,000	.0211 (.0276)	.1161 (.0675)
11,000≤ 1968 family income <14,000	-.0119 (.0552)	-.3359 (.1349)
14,000≤ 1968 family income <17,000	.0035 (.0677)	.4871 (.1656)
1968 family income ≥17,000	-.0029 (.0222)	-.0752 (.0544)

Regressions control for father's education, mother's education, rural origin, city origin, southern origin, number of siblings, age of 1968 head of household, and average family income in neighborhood of origin. Note that the dependent variables are not normally distribution so that standard t-tests do not apply. Standard errors are in parentheses.

Although the coefficient of average family income <8000 in neighborhood of origin was not quite significant at the 5 percent level for whites, it along with the significant coefficient for 11000≤ community income <17000, implies that the community income in both the lower and upper ranges exerted a positive influence on white sons' education. The effect for blacks was, however, limited to communities with average family incomes greater than or equal to $8000. The addition of the variable percentage white in the community in the second black regression equation lowered the coefficient of the significant community income variable by 30 percent. This suggests that much of the benefit that blacks seemed to receive from living in higher income communities actually resulted from living in more integrated neighborhoods.

Table 5.4 presents the results of the earnings regression analysis. The first two columns show the estimated effects of sons' characteristics omitting the background variables, while the remaining three include the background characteristics mentioned earlier. As indicated in the previous section, there are a number of reasons for exercising care in interpreting these coefficients. First, the coefficients of market-valued characteristics in the regressions omitting the background variables can be biased upwards because the included variables may be positively correlated with omitted market-valued characteristics. Ideally, adding the background variables would lower the amount of the bias because, to the extent that background also affects these omitted variables, it would act as proxies for them. Second, holding background constant, sons' market-valued characteristics are positively correlated with ability and holding market-valued characteristics constant, background is negatively correlated with ability. Therefore, in the regressions including background factors, the coefficients of market-valued characteristics are biased upwards and the coefficients of the background variables are biased downward. Third, many of the individuals in the sample, especially younger white men, may still have been in training on-the-job. As a result, the current earnings of these individuals would not accurately reflect their earnings capacity. In this case, the coefficients of both market-valued and background characteristics in the earnings regressions understate their impact on earnings capacity. In sum, these considerations suggest a downward bias in the size of the coefficients of the background variables and an indeterminate direction in the bias of the market-valued characteristics.

In light of these considerations, the following interpretations of the earnings regressions findings seem appropriate. As implied by the size of the coefficients of son's education in the first and second columns, years of education was a primary determinant of 1978 hourly wages. The higher coefficient for blacks indicates that, if black mean education increased by one year, the percentage gain as well as the absolute dollar amount of the increase in mean black wages would be higher than a similar change for whites. The coefficients of son's education in the regressions including background variables reported in columns 3 and 5 were almost a third lower than the coefficients in columns 1 and 2 respectively. This change substantially narrowed the hypothetical gain in mean earnings of blacks over whites due to an increase of one year in mean education. It implies that a large portion of the indicated return to education in columns 1

Table 5.4 (page 1 of 4)

RESULTS FOR 1978 LN HOURLY WAGES REGRESSIONS
(For employed male household heads aged 23-32.)

	Black	White	Black	Black	White
Son's education	.1441 (.0225) [.4797]	.0594 (.0123) [.2476]	.0988 (.0248) [.3291]	.1075 (.0246) [.3580]	.0406 (.0147) [.1691]
Self-employed	-.4428 (.5262) [-.0569]	-.8069 (.1012) [-.3970]	-.1957 (.4661) [-.0251]	-.2953 (.4667) [-.0379]	-.8571 (.1017) [-.4213]
Union member	.1007 (.0695) [.1039]	.1979 (.0686) [.1458]	.1803 (.0771) [.1860]	.1792 (.0777) [.1849]	.2160 (.0696) [.1591]
Durable manufacturing	.3051 (.0912) [.2507]	.2370 (.0746) [.1566]	.4349 (.0843) [.3573]	.4451 (.0848) [.3658]	.2730 (.0749) [.1802]
Years of work experience before current employer	.0264 (.0149) [.1673]	.0065 (.0112) [.0348]	.0098 (.0187) [.0621]	.0040 (.0186) [.0257]	.0136 (.0123) [.0728]
Years with current employer	.0367 (.0194) [.1775]	.0157 (.0135) [.0679]	.0041 (.0198) [.0200]	-.0020 (.0197) [-.0095]	.0200 (.0142) [.0868]

Table 5.4 (page 2 of 4)

	Black	White	Black	Black	White
Years for average person to become fully trained in current position	[illegible]	.0279 (.0123) [.1120]	.0213 (.0275) [.0498]	.0188 (.0277) [.0440]	.0179 (.0125) [.0716]
Father's education			.0001 (.0147) [.0010]	.0033 (.0148) [.0225]	.0090 (.0130) [.0529]
Mother's education			.0395 (.0159) [.2650]	.0353 (.0159) [.2370]	-.0022 (.0154) [-.0102]
Rural origin			-.3039 (.1558) [-.1568]	-.2977 (.1570) [-.1536]	.1897 (.1012) [.0988]
City origin			-.2175 (.0835) [-.2211]	-.2355 (.0836) [-.2395]	.1527 (.0589) [.1316]
Southern origin			1032 (.0959) [.1054]	.1383 (.0947) [.1412]	.1595 (.0824) [.1028]

Table 5.4 (page 3 of 4)

	Black	White	Black	Black	White
Number of siblings			.0027 (.0140) [.0140]	.0086 (.0138) [.0445]	-.0348 (.0140) [-.1266]
Age of 1968 head of household			.0178 (.0042) [.3168]	.0181 (.0042) [.3217]	-.0017 (.0042) [-.0211]
1968 family income <5000			.0717 (.0472) [.1333]	.0541 (.0470) [.1005]	
5000 ≤1968 family income <8000			-.1668 (.0447) [-.4722]	-.1513 (.0442) [-.4283]	
1968 family income ≥8000			.0197 (.0215) [.1135]	.0132 (.0213) [.0759]	
1968 family income					.0083 (.0054) [.0793]
1969 community income <8000			-.0177 (.0696) [-.0238]	-.0091 (.0700) [-.0122]	.0327 (.0878) [.0219]

Table 5.4 (page 4 of 4)

	Black	White	Black	Black	White
8000≤ 1969 community income <11,000			.0694 (.0606) [.1579]	.1206 (.0541) [.2745]	-.0228 (.0425) [-.0383]
1969 community income ≥11,000			-.0217 (.0661) [-.0323]	-.0359 (.0662) [-.0535]	
11,000≤ 1969 community income <14,000					.0637 (.0333) [.1300]
1969 community income ≥14,000					-.0194 (.0161) [-.0646]
1970 percentage white in community			.0028 (.0015) [.1604]		
Intercept	-.5348	.7658	-1.1441	-1.1856	.6298
R^2	.3255	.2782	5577	.5470	.3308
N	158	329	158	158	329

Standard errors are in parentheses. Standardized coefficients are in brackets.

and 2 was, in fact, due to more favorable backgrounds independent of differences in education.

The same was true for the experience and training variables. With the addition of the background factors, the coefficient of years with current employer dropped by almost 90 percent for blacks and the coefficient of years to become fully trained fell by over a third for whites. Although both of these were positive and statistically significant in the earnings regressions omitting the background variables, neither was significant in columns 3 or 5.

There was little change between columns 2 and 5 in the coefficients of job characteristics for white men. Whether self-employed remained negative, while union member and employed in durable manufacturing continued to be large and positive. There were too few black self-employed in the sample to obtain precise estimates for this variable. However, the coefficients of both union membership and employed in durable manufacturing jumped substantially with the addition of the background factors. This change was consistent with market-valued characteristics being biased upward by the addition of background factors in the earnings regression.

Looking at the coefficients of the background variables in the white earnings regression showed that only number of siblings, city origin, and increments of community income in $11,000-$14,000 range were statistically significant. As expected, the first of these was negative while the other two were positive. As mentioned earlier, a possible reason for the failure of the other background variables to affect wages of white sons is that many young white men may have been foregoing a portion of their earnings to engage in on-the-job training. If this were true, the coefficients of the background variables should be higher for older men since they would be most likely to have completed their training. Columns 1 and 2 of Table 5.5 list the coefficients of the background variables when a dummy variable for age >29 and interaction terms for father's education and community income are introduced into the equation.[2] It shows that both interaction terms are significant and positive.

Reduced form earnings regressions for white men with age interaction terms are listed in columns 3-5 of Table 5.5. The results paralled those shown in columns 1 and 2, except that years of mother's education was also significant and positive. Thus, it is clear that, consistent with the hypothesis of younger men

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[2] Both interaction terms were not added into the same regression because of a high degree of multicollinearity.

Table 5.5 (page 1 of 2)

RESULTS FOR 1978 LN HOURLY WAGES REGRESSIONS
USING AGE INTERACTION TERMS[1]
(For employed white male household heads aged 23-32.)

	(1)[2]	(2)[2]	(3)[3]	(4)[3]	(5)[3]
Father's education	-.0024 (.0136) [-.0141]	.0058 (.0130) [.0342]	-.0129 (.0153) [.0024]	.0004 (.0147) [.0024]	-.0053 (.0147) [-.0312]
Mother's education	-.0033 (.0153) [-.0152]	.0010 (.0153) [.0047]	.0095 (.0176) [.0441]	-.0054 (.0197) [-.0252]	.0145 (.0176) [.0677]
Rural origin	.1783 (.1005) [.0928]	.1631 (.1000) [.0849]	.1307 (.1166) [.0680]	.1650 (.1174) [.0859]	.1178 (.1167) [.0613]
City origin	.1282 (.0593) [.1105]	.1450 (.0587) [.1250]	.1309 (.0689) [.1128]	.1479 (.0686) [.1275]	.1448 (.0682) [.1248]
Southern origin	.1389 (.0824) [.0895]	.1438 (.0822) [.0927]	.0876 (.0938) [.0565]	.0949 (.0942) [.0611]	.1021 (.0932) [.0658]
Number of siblings	-.0344 (.0140) [-.1250]	-.0334 (.0140) [-.1212]	-.0314 (.0157) [-.1142]	-.0310 (.0158) [-.1126]	-.0287 (.0157) [-.1044]
Age of 1968 head of household	-.0016 (.0042) [-.0204]	.0001 (.0043) [.0003]	-.0005 (.0047) [-.0063]	-.0016 (.0048) [-.0198]	.0010 (.0048)].0131]
1968 family income	.0099 (.0054) [.0943]	.0089 (.0054) [.0855]	.0086 (.0063) [.0822]	.0076 (.0063) [.0724]	.0076 (.0063) [.0726]

Table 5.5 (page 2 of 2)

	(1)[2]	(2)[2]	(3)[3]	(4)[3]	(5)[3]
1969 community income <8000	.0246	-.0062	-.0189	.0059	-.0578
	(.0876)	(.0888)	(.1021)	(.1025)	(.1036)
	[.0165]	[-.0041]	[-.0126]	[.0040]	[-.0387]
8000$\leq$1969 community income <11,000	-.0245	-.0301	-.0053	-.0028	-.0095
	(.0425)	(.0425)	(.0491)	(.0494)	(.0490)
	[-.0411]	[-.0506]	[-.0088]	[-.0047]	[-.0159]
11,000$\leq$1969 community income <14,000	.0514	.0390	.0349	.0436	.0188
	(.0335)	(.0343)	(.0391)	(.0389)	(.0402)
	[.1049]	[.0795]	[.0714]	[.0889]	[.0383]
1969 community income$\geq$14,000	-.0162	-.0196	.0038	.0017	-.0012
	(.0161)	(.0160)	(.0186)	(.0186)	(.0185)
	[-.0541]	[-.0653]	[.0127]	[.0058]	[-.0041]
Age >29	-.5498	-.7889	-.5921	-.5417	-.9055
	(.2280)	(.3036)	(.2527)	(.3020)	(.3427)
	[-.3995]	[-.5732]	[-.4303]	[-.3936]	[-.6580]
Age >29* father's education	.0523		.0564		
	(.0195)		(.0226)		
	[.4220]		[.4547]		
Age >29* community income		.0658			.0750
		(.0238)			(.0274)
		[.5944]			[.6774]
Age >29* mother's education				.0508	
				(.0270)	
				[.4175]	
Intercept	.8272	.9639	1.80707	1.65744	1.93198
R^2	.3471	.3480	.0704	.0626	.0740

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[1] Standard errors are in parentheses. Standardized coefficients are in brackets.

[2] These equations control for son's education, years of work experience before current employer, years with current employer, years for average person to become trained in current position, whether self-employed, whether union member, and whether employed in durable manufacturing.

[3] These are reduced form equations and include only the variables listed.

foregoing earnings to acquire on-the-job training, background characteristics--father's education, mother's education, and community income--had a stronger effect on the earnings of older men than younger men.

Looking at the coefficients of the background variables for black men in Table 5.4 shows that mother's education and age of 1968 head of household were positive and statistically significant at the 5 percent level in both columns and that rural origin, city origin, and increments in family income in the $5000 to $8000 range were negative and significant. Although gains in community income from $8000 to $11,000 raised son's earnings in column 4, the inclusion of percentage white in the community in column 3 substantially lowered the effect. This is consistent with the earlier finding in the education regressions which indicated that much of the benefit that blacks seemed to receive from living in higher income communities actually resulted from living in more integrated neighborhoods. The positive signs of mother's education and age of 1968 head were predicted in the earlier theoretical discussion. However, the large size of the mother's education coefficient relative to the size of the father's education coefficient was not expected. It suggests that, while the benefit that sons received through higher father's education operates through the characteristics taken into account in the regression (e.g., son's education), the mother's educational experience affected other unmeasured market-valued characteristics. The negative sign for rural origin implies that the advantage rural migrants had over individuals from small towns or large cities noted earlier was limited to educational attainment. Holding constant education, taste and/or information differences between the groups may make rural migrants appear less well off.

The two most puzzling findings were the negative signs of city origin and of family income in the $5000-$8000 range. The sign of city origin indicated that individuals who spent most of their early lives in large cities earned less than individuals from small towns. This finding may reflect the negative externalities that blacks who lived in central city areas endured. The negative sign for the income variable can be explained by the downward bias in the background characteristics caused by the omission of son's ability. If sons whose families had incomes in this range achieved higher earnings because they remained in school longer or they had easier access to high-paying union and manufacturing jobs, then only those sons with lower ability would have the same levels of these characteristics as sons whose families had less income. Holding these characteristics constant, family income in the $5000-$8000 range would then be correlated with lower ability. The reduced form earnings equation for blacks in Table 5.6 suggests that this interpretation may account for much of the negative effect of the family income. When education, union membership, and employment in durable manufacturing were not controlled for, the coefficient of family income $5000-$8000 was not significantly different from zero even at the 10 percent level.

Table 5. page 1 of 2)

RESULTS FOR REDUCED FOR '78 LN HOURLY WAGES REGRESSIONS
(For employed black m household heads aged 23-32.)

	Black	Black
Father's education	.0388 (.0149) [.2669]	.0327 (.0148) [.2254]
Mother's education	.0207 (.0175) [.1387]	.0287 (.0175) [.1926]
Rural origin	-.1284 (.1668) [-.0662]	-.1696 (.1645) [-.0875]
City origin	-.2573 (.0891) [-.2616]	-.2339 (.0879) [-.2378]
Southern origin	.0702 (.0877) [.0717]	.0328 (.0873) [.0334]
Number of siblings	.0076 (.0158) [.0394]	-.0020 (.0160) [-.0102]
Age of 1968 head of household	.0216 (.0045) [.3855]	.0212 (.0044) [.3783]
1968 family income <5000	.0571 (.0547) [.1061]	.0827 (.0546) [.1537]
5000≤ 1968 family income <8000	-.0502 (.0438) [-.1421]	-.0700 (.0437) [-.1983]
1968 family income ≥8000	-.0237 (.0218) [-.1367]	-.0154 (.0217) [-.0884]

The coefficients of the black background variables in the earnings regressions should be biased downwards because of on-the-job training the same as in the case of whites. However, since blacks engaged in less on-the-job training, the extent of the bias should be smaller for blacks than whites. The black earnings regressions which included age interaction terms all had insignificant coefficients for these terms. This is consistent with a hypothesis of little difference between younger and older blacks in the extent of earnings

Table 5.6 (page 2 of 2)

	Black	Black
1969 community income <8000	-.0691	-.0818
	(.0809)	(.0796)
	[-.0928]	[-.1099]
8000≤ 1969 community income <11,000	.1281	.0483
	(.0620)	(.0685)
	[.2915]	[.1099]
1969 community income ≥11,000	-.0774	-.0531
	(.0748)	(.0741)
	[-.1152]	[-.0791]
1970 percentage white in community		.0044
		(.0017)
		[.2512]
Intercept	.4297	.3712
R^2	.3443	.3727

Standard errors are in parentheses. Standardized coefficients are in brackets.

foregone through on-the-job training and thus with little training being undertaken by blacks.

Although the discussion so far has provided some evidence concerning the importance of family and community background in determining the education and earnings of black and white men, it would be useful in addition to indicate exactly how much difference it makes that blacks come from less favorable backgrounds than whites. Table 5.7 computes the expected change in black mean education and ln hourly wages if black backgrounds were identical to white backgrounds. For each of the background variables which had a significant effect on education or earnings, the table shows that difference in the impact of the variable if blacks had white mean values of the variable rather than their actual levels. The computations were based on the assumption that the process through which background factors influence black education and earnings remain unchanged.[3] According to the table, the level of black mean education would be

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[3] For each variable, X_i, which had a significant impact on black years of education, the recorded figure in column 1 of Table 5.7 equals $\beta_i(\overline{X}_i^W - \overline{X}_i^W)$ where β_i is the coefficient of X_i in the wage equation in column 3 of Table 5.4, $\overline{X}_i^W$ is the mean of X_i for whites, and $\overline{X}_i^B$ is the mean of X_i for blacks. The total hypothetical change in black mean education is $\Sigma_i \beta_i (\overline{X}_i^W - X_i^B)$. The earnings figures are calculated in a similar way.

Table 5.7

SUMMARY OF THE EFFECTS OF BACKGROUND DIFFERENCES
BETWEEN BLACKS AND WHITES ON EDUCATION AND LN HOURLY WAGES
(For employed male heads of households aged 23-32.)

	Education Effect	Ln Wage Effects		
	Effect on Years of Sons' Education	Indirect Effect on ln Hourly Wage through Sons' Education	Direct Effect on ln Hourly Wage	Total Effect on ln Hourly Wage
Total	1.07	.11	.10	.21
Father's education	.1536	.0152	-	.0152
Mother's education	-	-	.0306	.0306
Rural origin	.0603	.0060	-.0107	-.0047
City origin	-.0821	-.0081	.0242	.0161
Southern origin	-	-	-	-
Number of siblings	-	-	-	-
Age of 1968 head of household	.0751	.0074	.0249	.0323
1968 family income <8000	-	-	-	-
1968 family income ≥8000	-	-	-	-
1968 family income <5000	-	-	-	-
5000< 1968 family income ≤8000	-	-	-.0995	-.0995
1969 community income <8000	-	-	-	-
8000≥ 1969 community income <11,000	-	-	-	-
1969 community income ≥8000	.3930	.0388	-	.0388
1969 community income >11,000	-	-	-	-
1970 percentage white in community	.4755	.0470	.1305	.1775

slightly more than one year higher, while having the same background as whites would raise hourly wages by 21 percent. In both cases, the principal contributors to higher black attainment were the community background factors. They accounted for 75 percent of the contribution of background factors that raised son's education and 70 percent of the contribution of background factors that raised son's earnings. While father's education was the main non-community contributor to son's education, mother's education and age of head played the largest roles in determining earnings.[4] Note that, because of reasons mentioned earlier, the coefficients of the variables in the structural earnings education may be biased. As a result, the estimates of the direct effect of the background variables on earnings are lower bound figures, while the indirect effect through education may be somewhat inflated.

SUMMARY

The primary purpose of this paper was to determine the effect of background on the education and earnings of black and white men. It was largely motivated by a desire to quantify the extent to which past discrimination against blacks resulting in lower achievement inhibits the progress of individuals today in a somewhat more benign environment. It has demonstrated that for our sample both community and family background factors were important in determining the levels of education and earnings of black and white men. The community effects for blacks operate largely through their moving into more integrated neighborhoods so that many positive community externalities were apparently not available to families in predominantly black middle-class neighborhoods. While the effects of father's education, city origin, and community income were comparable between blacks and whites, white men's education was more affected by number of siblings, family income, and age of 1968 head of household than was black men's education. The relative sizes of the coefficients of these latter variables were consistent with steeper age/earnings profiles for older white men than older black men and higher prices paid for investing in children by black parents.

The findings for the earnings regressions imply that (1) the omission of background factors biases upwards the effects of education, experience, and training variables, (2) failure to acknowledge foregone earnings for younger men will lead to underestimates of the effects of background and possibly other variables (e.g. education and training) on earnings capacity, and (3) the omission of ability may bias downwards the impact of background and bias upward the effect of acquired characteristics.

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[4] When the coefficients of the earnings regression omitting percentage white in neighborhood of origin were used, the results were almost identical to these findings.

The final summary table shows that differences in backgrounds between blacks and whites have large effects on achievement and that the principal source of the differences stem from differences in the type of community rather than from variations in individual family backgrounds.

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Chapter 6

THE INCIDENCE AND SOME CONSEQUENCES OF MAJOR LIFE EVENTS[1]

Greg J. Duncan and James N. Morgan

INTRODUCTION

Research on aging by psychologists, sociologists, and biologists is beginning to coalesce into a "life-course" perspective on human development.[2] The emerging concept of a life course is much more than a minor extension of the "life-cycle" concept. It symbolizes an ultimate goal far more ambitious--a dynamic modeling of the courses of individuals through their lifetimes. The models are complex since biological processes of aging are found to both affect and be affected by the development of knowledge, attitudes, behavior, and attainment.

The benchmarks in the life course analysis are a variety of life events, and yet very little is known about the occurrence of these events. Past studies have typically focused on the effects of a subset of events on a small (and often unrepresentative) subset of the population. Data from the Panel Study of Income Dynamics encompass a large number of economic and demographic events, gathered from a sample of individuals that is representative of the population as a whole. A panel study supplies better data on the frequency of events because it provides a count of the number of times various events occur over a multi-year (in this case 11 years) time period rather than over a single year. In this chapter, we use the Panel Study data to describe the frequencies of different events by chronological age, to describe the intercorrelation of events, to explore the determinants of events, and to search for possible effects of events. The data are weakest in the outcome measures: here we use change in the measures of efficacy and of future orientation as outcomes.

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[1] The computer work associated with this chapter was done by Sue Augustyniak and supported by a grant from the Social Science Research Council's Committee on Life-Course Perspectives on Middle and Old Age.

[2] For a brief exposition of the life course perspectives, see Abeles and Riley (1977).

The analysis of Panel Study data on life events presented here is preliminary and quite descriptive. We include it to inform others of the potential of Panel Study data in life course research and to present the basic descriptive information that ought to be of use in future life course research.

Analysis

The list of life events constructed from the Panel Study data follow from the work of Sarason, et al. (1978). The total list of available measures is given in Table 6.1, with definitions spelled out in Table 6.2.[3] We have classified and indexed some of the events as "undesirable" and some as "desirable"; the remainder fall into the "ambiguous" category.[4] As can be seen from the table, the list covers a wide variety of events, ranging from economic (changes in income and work hours, retirement, major unemployment, and involuntary job loss), to demographic (changes in marital status, residential location, and family composition), to health-related (disability, work loss due to illness).

Since Panel Study procedure calls for interviews with household heads, we have the most extensive and reliable information about them. A list of events available for each group of sample individuals is given in Table 6.3. The actual numbers entered in that table are ordinal measures of association (Tau-B) between the number of times each particular event occurred and the age of the individuals to whom it occurred. Ages are bracketed into ten-year intervals. No entry appears on Table 6.3 when the measure of events cannot be constructed for that particular subset of individuals. Information on family demographic events (changes in marital status and family size) is available for virtually all sample individuals. Three additional events were meacured for those who began the panel period as children living in their parental homes: splitting off from the parental home, stopping school, and the addition of step parents. Most of the labor market events were measured only for the heads of households.

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[3] The OSIRIS IV Recode statements and variable numbers used to construct these events are available from the authors upon request. The construction should be transparent to those unfamiliar with OSIRIS.

[4] The events "Any Job Change" and "Any Residential Move" were included because the voluntary and involuntary classifications do not exhaust all types of job changes and moves. The indexes of total events include all job and residential changes.

Table 6.1

CLASSIFICATION OF EVENTS

Desirable Events (presumably mostly voluntary)

- Marriage or Remarriage
- Voluntary Job Change
- Major Increase in Family Income
- Voluntary Residential Move

Undesirable Events (presumably mostly involuntary)

- Becoming Widowed
- Divorce or Separation
- Major Unemployment
- Involuntary Job Change
- Major Work Loss Due to Illness
- Becoming Disabled
- Involuntary Residential Move
- Major Decrease in Family Income
- Loss of Parent

Ambiguous Events

- Decrease in Family Size
- Increase in Family Size
- Major Increase in Labor Income
- Major Decrease in Labor Income
- Major Increase in Work Hours
- Major Decrease in Work Hours Other than Retirement
- Retirement
- Birth of a Child
- Any Job Change
- Any Move
- Addition of Step-Parent
- Separation from Parental Home
- Stopping School

Indexes of Events

- The Total Number of Events
- The Total Number of Different Events
- The Total Number of Desirable Events
- The Number of Different Desirable Events
- The Total Number of Undesirable Events
- The Number of Different Undesirable Events

Table 6.2 (page 1 of 3)

DEFINITION OF EVENTS

MARRIAGE OR REMARRIAGE—A count of the number of times in year t+1 a respondent reported that he or she was married when in year t he or she had not been married or had been married to a different spouse. (Maximum possible score is 10.)

VOLUNTARY JOB CHANGE--In the first year a job change was counted as voluntary if the respondents had been working for their present employer for less than 18 months, and said that previously they had been self-employed, had quit, resigned, retired, or become pregnant. In subsequent years only those respondents who had changed jobs for those same reasons since the last interview scored one on this variable. (Maximum score is 11.)

MAJOR INCREASE IN FAMILY INCOME—The total family income variable is the sum of the following variables: taxable income of head and wife, total transfers of head and wife, taxable income of others, and total transfers of others. Total family income increases were considered major if this variable had doubled since the previous year. (Maximum possible score is 10.)

VOLUNTARY RESIDENTIAL MOVE--If the respondent stated that the reason for the move was productive (to get nearer to work), consumptive (a change in the house size) or to save money then the move was considered voluntary. (Maximum possible score is 10.)

BECOMING WIDOWED—A count of the number of times a respondent replied that he or she had become widowed since the previous interview. (Maximum possible score is 10.)

DIVORCE OR SEPARATION--The number of times respondents reported that they were divorced or separated after having been married either at the time of or since the last interview. (Maximum possible score is 10.)

MAJOR UNEMPLOYMENT--The sum of a person's annual hours on strike and unemployed was checked. If these annual hours exceeded 174, approximately one month of workdays, that year was counted as one in which there had been major unemployment. (Maximum possible score is 11.)

INVOLUNTARY JOB CHANGE--The currently unemployed and those employed at their present job for one year or less were asked what had happened to their previous job. If they replied that the company had gone out of business, that they were on strike, or had been laid off, it was considered an involuntary job change. (Maximum possible score is 11.)

MAJOR WORK LOSS DUE TO ILLNESS--If the number of working days lost to illness reported during the year exceeded one month, then it was considered a major illness. The days ill variable is the sum of work hours missed because of someone elses illness and the respondent's own days ill. (Maximum possible score is 11.) (The average number of hours of own illness is five times higher than the average number of hours missed due to the illness of others. For men, the ratio is ten to one.)

Table 6.2 (page 2 of 3)

BECAME DISABLED--Respondents who reported that they were disabled either completely or severely in one interview when they were not disabled in the previous interview were counted as becoming disabled. For the years 1973-1975 the question was not asked of all respondents so these years were not used. For the years 1969-1971 the question did not differentiate between completely, severely or slightly disabled. (There were eight reports on disability status. The maximum possible score on this measure is four.)

INVOLUNTARY RESIDENTIAL MOVE--A move was considered involuntary if it was in response to outside events; such as eviction, divorce, health reasons, or the armed services. (Maximum possible score is 10.)

MAJOR DECREASE IN FAMILY INCOME--If the total family income variable decreased to less than half of its value in the previous year, this was counted as a major decrease in family income. (Maximum possible score is 10.)

LOSS OF A PARENT--A count of the times an individual resided in a family which had lost either its head or wife through divorce, separation, or death. (Maximum possible score is 11.)

DECREASE IN FAMILY SIZE--This variable is defined by negative net changes in family size. In 1968 the codes for people moving in or out of the family were truncated at four or more. If four or more people both moved in and moved out any net change there has been lost. (Maximum possible score is 11.)

INCREASE IN FAMILY SIZE--Any year in which the net change in family size was positive was counted as an increase. Here again the 1968 codes were truncated at four or more and some net change may have been lost. (Maximum possible score is 11.)

MAJOR INCREASE IN LABOR INCOME--There was a major increase in labor income from year t to t+1 if year t income was $1,000 or more and income in t+1 was more than twice as large as income in year t. (Maximum possible score is 10.)

MAJOR DECREASE IN LABOR INCOME--If labor income in year t+1 was less than half of the amount of labor income in year t, it was considered a major decrease, providing the income had been greater than $1,000 in year t. (Maximum possible score is 10.)

MAJOR INCREASE IN WORK HOURS--An increase in work hours over the previous year of 500 hours or more. Where work hours of respondents who had been retired in the previous year increased 500 hours or more they were not considered to have had a major increase in work hours.

MAJOR DECREASE IN WORK HOURS--A decrease of more than 500 hours in one year. A decrease in hours caused by retirement was not included.

RETIREMENT--If a respondent reported labor force status as unemployed or employed in year t and retired in year t+1, then he or she was counted as retiring. (Maximum possible score is 5.)

BIRTH OF CHILDREN--This is defined as a reported change in household composition involving someone other than head or wife together with a reported increase

Table 6.2 (page 3 of 3)

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in family size and a response that the age of the youngest child was less than one year. For 1968 and 1969 we only have the category age of the youngest child is less than 2. (Maximum possible score is 10.)

ANY JOB CHANGE--In 1968, if the respondent had been working less than 18 months at his present job, it was considered a job change. In subsequent years a response of working at the present job for less than one year was considered a job change. (Maximum possible score is 11.)

ANY MOVE--Any time the respondent reported that there had been a residential move for any reason during the past year. (Maximum possible score is 10.)

ADDITION OF STEP PARENT--If a respondent reported that a child lived in the household with a married parent in year t+1, and that child had lived with a parent who had not been married or a parent who had been married to a different person in year t, then that year was counted as an addition of a step parent. (Maximum possible score is 11.)

SEPARATION FROM PARENTAL HOME--The number of instances in which an individual was classified as a child or grandchild in year t followed by a classification of a head or wife in year t+1. (Maximum possible score is 10.)

STOPPING SCHOOL--Any year in which the respondent reported that an individual had stopped going to school during the year. (Maximum possible score is 10.)

THE MAJOR LIFE EVENTS OF MARRIED MEN

The Appendix to this chapter includes 28 different tables on the life events of the initially married men who headed households in 1968. The first 22 tables contain information on one event only, and their order follows the order of events listed in Table 6.1. Each of these tables shows by age a frequency distribution of the number of times each event occurred to the respondent, as well as the mean number of events and their range. Age is bracketed into ten year intervals except for the two five-year periods on either side of age 65. The tables also list the number of the sample individuals who fell into each age category and the weighted proportion of observations in each. All of the numbers in the tables are weighted to adjust for differential sampling and nonresponse rates. Two measures of the ordinal association between age and the frequency of each event are given at the bottom of each table. Both the Tau-B and Gamma measures range between -1 and +1 and show the direction and magnitude of the association. (Each of these measures can be thought of as the ordinal analogue to a zero-order correlation coefficient or a rank correlation, or the proportion of all possible pairs where the observation which is higher on one rank is also higher [lower] on the other.)

The last six appendix tables summarize information on the different sets of life events. Table A6.23 is based on a count of the total number of events

Table 6.3 (page 1 of 2)

ORDINAL ASSOCIATION BETWEEN VARIOUS LIFE EVENTS AND AGE

	Married Male Household Heads in 1968	Wives in 1968	Unmarried Male Household Heads in 1968	Unmarried Female Household Heads in 1968	Children under 10 in 1968[a]	Children 10-30 in 1968	Other (mostly adult relatives of the head)
Marriage or remarriage	-.138	-.160	-.403	-.395	.064	.325	
Voluntary job change	-.361		-.515				
Major increase in family income	-.017	-.046	-.300	-.165	-.117	.065	-.109
Voluntary residential move	-.335	-.343	-.502	-.353	-.295	.236	-.278
Becoming Widowed	.196	.296		.062		.013	
Divorce or separation	-.154	-.176	-.070	-.229		.143	
Major unemployment	-.166		-.123				
Involuntary job change	-.207		-.151				
Major work loss due to illness	-.007		.116				
Becoming disabled	.284	.315	.273	.188	-.020	.069	
Involuntary residential move	-.126	-.065	-.107	-.140	-.078	.094	-.120
Major decrease in family income	.068	.021	.129	.007	-.063	.019	-.052
Decrease in family size	.040	.042	.067	-.171	.387	-.223	-.227
Increase in family size	-.335	-.357	-.359	-.398	-.161	.249	-.267
Major increase in labor income	-.139	-.172	-.414	-.298			
Major decrease in labor income	.247	-.179	.258	-.152			
Major increase in work hours	-.311	-.326	-.436	-.394			
Major decrease in work hours other than retirement	-.231	-.285	-.293	-.384			

Table 6.3 (page 2 of 2)

	Married Male Household Heads in 1968	Wives in 1968	Unmarried Male Household Heads in 1968	Unmarried Female Household Heads in 1968	Children under 10 in 1968[a]	Children 10-30 in 1968	Other (mostly adult relatives of the head)
Retirement	.453		.526				
Birth of a child	-.477	-.501	-.444	-.382		.268	
Any job change	-.412		-.606				
Any move	-.323	-.308	-.518	-.340	-.282	.239	-.267
Addition of step parent					-.153	-.079	
Separation from parental home					.055	.306	
Stopping school					.179		
Loss of a parent					-.084	-.073	
Total number of events	-.272	-.315	-.425	-.424	-.045	.213	-.226
Number of different events	-.216	-.279	-.286	-.416	-.105	.183	-.190
Total number of desirable events	-.351	-.282	-.545	-.352	-.270	.254	-.262
Number of different desirable events	-.341	-.257	-.595	-.349	-.270	.241	-.280
Total number of undesirable events	-.025	.182	-.009	.022	-.110	.062	.081
Number of different undesirable events	-.015	.191	.011	.011	-.100	.063	.043

[a] Tau-B's for children less than 10 years old show association between events and age of mother.

experienced by the married male respondents. In this table, two occurrences of the same kind of event would count as a total of two events. In Table A6.24, the number of different events is calculated and related to age. In this table, two occurrences of a single event would count only once. The remaining tables show the total number of desirable and undesirable events as well as the number of different desirable and undesirable events.

Each reader may be surprised and impressed by different findings culled from these tables. Our list of highlights is as follows:

1. Few of the married men in the panel lived through the 11-year period without experiencing a substantial number these life events. More than 99 percent of this sample had at least one event occur to them, less than one-third of the group escaped undesirable events, and about the same proportion failed to experience at least one desirable event. The average numbers of total, desirable, and undesirable events were 10.2, 2.0, and 2.4, respectively. The average number of different types of total, desirable, and undesirable events was 5.9, 1.2, and 1.5, respectively.[5]

2. A systematic story of life patterns appears in these tables. Most of the action takes place among young people, and with respect to many events, the process of settling down to no change is rather rapid. Voluntary changes in job or residential location and the births of children were especially concentrated among respondents in their 20s. Involuntary job and residential changes and unemployment had a somewhat more uniform distribution across the age ranges. Some divorces and remarriages were observed within each age group, although both were most frequent among the young. Divorces were least likely among married men in their 50s and became somewhat more likely to occur among those older than 60. Except for income decreases that accompany retirement, major changes in family income in either direction were almost completely independent of age. Only the incidence of widowhood, disability, and retirement increased with age. In the middle years, time lost from work because of illness peaked, and decreases in family size were most apt to occur, as children left home. All in all, these age patterns suggest that recent attention to "mid-life" changes is focusing on exceptions rather than common occurrences.

3. The patterns were uniformly accentuated when multiple occurrences of the same event were tallied. Multiple moves and multiple job changes were even more

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[5] The mean number of desirable and undesirable events do not add to the mean number of total events because of the ambiguous events that classified as neither desirable nor undesirable.

frequent among the young than moving or changing jobs at all. For this reason, for any one individual there were high correlations between recent past changes in jobs or residential location and the probability of a future change, even if the changes did not affect one another. Recent sophisticated attempts to separate any real effect of an event on later events (state dependence) from spurious correlation of two events through age or other individual differences (heterogeneity) find relatively little genuine causal effect across time. (See Heckman (1978) and the Corcoran and Hill chapter in this volume.)

4. Undesirable events were frequent, common, and persistent through the life course. No more than one-third of the married men in any of the age groups escaped completely from undesirable events during this 11-year period. Generally, the few things which are more frequent as one grows older, such as disability and widowhood, are considered undesirable. Combining these events with unemployment, involuntary residential changes, and divorce--which decrease in frequency over the life course--produces a flat pattern: some undesirable events occur at all ages.

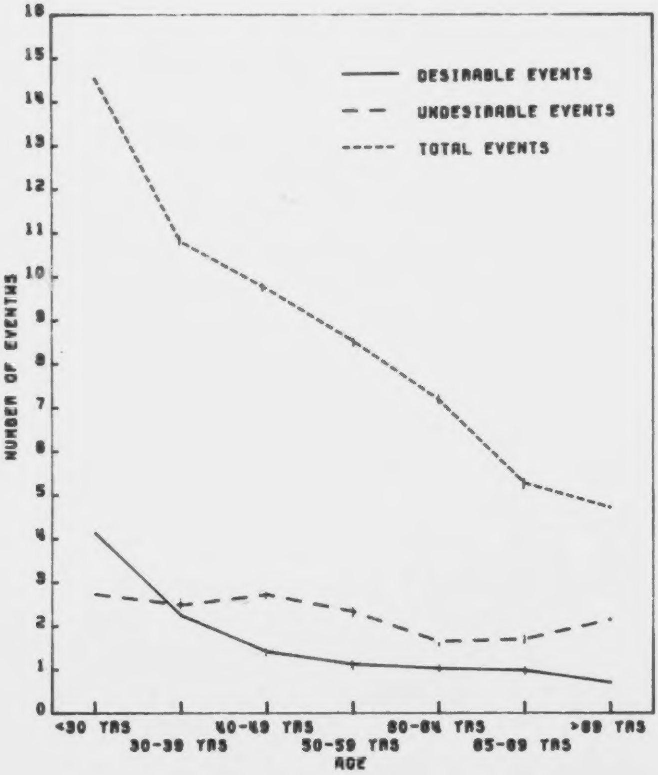
LIFE EVENTS OF OTHER SAMPLE INDIVIDUALS

Rather than detail the frequency of all events measures for each of the groups of sample individuals, we summarize this information in several ways. As mentioned above, Table 6.3 presents the ordinal association between each of the events and age, as indicated by the Tau-B statistic. Figures 6.1 to 6.3 show the frequency of undesirable, desirable, and total events by age in three of the major groups of adults--initially married men, wives, and female household heads.

The age patterns summarized in Table 6.3 were quite consistent across most of the groups of sample individuals, especially the adults. Changes in residence, job, and marital status (except becoming widowed) occurred most frequently to the young. The frequency of disability, retirement, and widowhood increased with age. When we combined the events into the indexes, we found that both total events and desirable events declined with age. Undesirable events, on the other hand, had virtually no association with age. The only exception to this was among wives, for whom the frequency of widowhood and disability caused the overall association between age and undesirable events to be positive. The association between the events and the age of the mother are shown for young children. Since the age range was restricted considerably, most associations were found to be smaller in absolute value than they were for the adult group.

Figure 6.1

TOTAL DESIRABLE AND UNDESIRABLE
EVENTS BY AGE
(For all married male household
heads in 1968.)

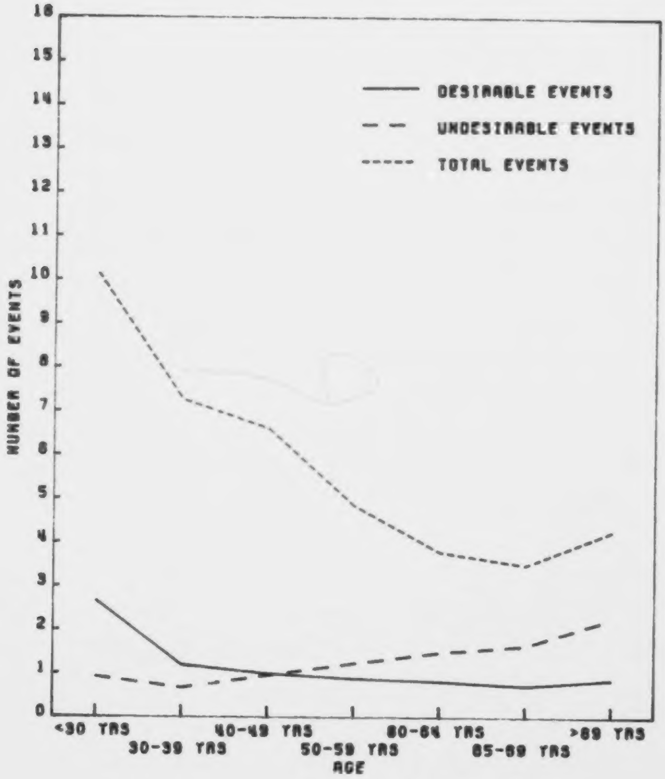


INTERCORRELATIONS AMONG EVENTS

The extent to which different events occurred to the same individuals was explored through the simple correlations shown in Table 6.4. The sample consisted of initially married men, and separate correlations were estimated for several ten year-age cohorts, as indicated at the bottom of this table. Only correlations with absolute values larger than .2 are shown on the table. (The

Figure 6.2

TOTAL DESIRABLE AND UNDESIRABLE
EVENTS BY AGE
(For wives in 1968.)

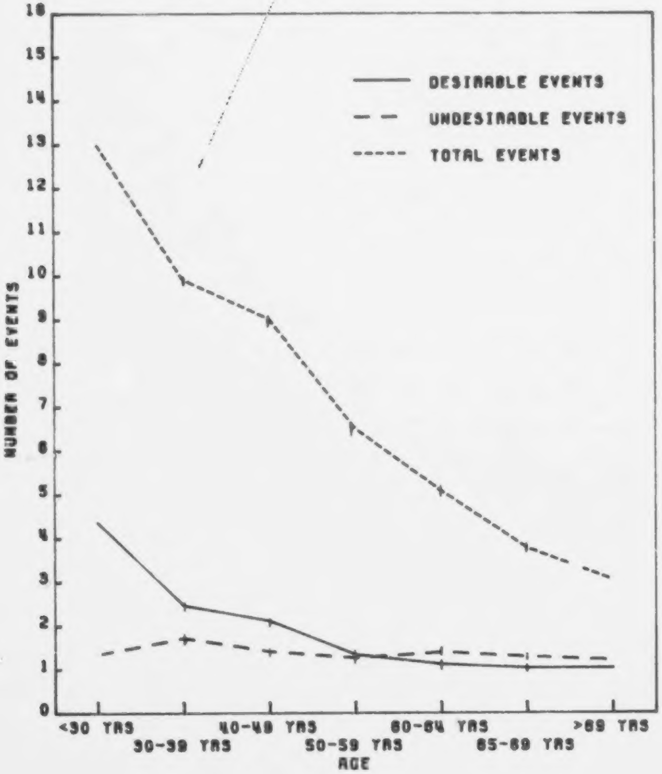


one percent significance level for correlation of samples of these sizes is approximately .2.)

In general, different events did not tend to occur together to the same individuals, as indicated by the fact that most entries in the table are blank. Most of the patterns of intercorrelations that are entered in the table were to be expected: divorces and remarriages tended to go together, as did involuntary job changes and unemployment. Some of the substantial correlations were semi-

Figure 6.3

TOTAL DESIRABLE AND UNDESIRABLE
EVENTS BY AGE
(For unmarried female household
heads in 1968.)



definitional. Increases and decreases in family income were of this type, as were the two marital status changes and the two types of residential moves.

It is interesting to note that none of the events had negative intercorrelations. A few events tended to occur together but none became less frequent as others became more frequent.

Table 6.4 (page 1 of 3)
SIMPLE CORRELATION BETWEEN EVENTS
(For all married male household heads in 1968.)

	Number of Remarriages	Voluntary Job Change	Increase in Family Income	Voluntary Residential Move	Number of Widowings	Number of Divorces	Number of Times Unemployed
Voluntary job change							
Increase in Family Income	.29						
Voluntary residential moves	.20 .35 .26	.31 .25	.22				
Number of times widowed	.52 .42						
Number of divorces	.66 .67 .59 .66		.25	.25 .30 .30			
Number of times unemployed							
Involuntary job change		.21		.27			.47 .44 .42 .42 .21

Table 6.4 (page 2 of 3)

	Number of Remarriages	Voluntary Job Change	Increase in Family Income	Voluntary Residential Move	Number of Widowings	Number of Divorces	Number of Times Unemployed
Illness hours							.27
							.21
Disability							
Involuntary residential move	.27					.30	
	.38			.21		.44	
	.24					.41	
Decrease in family income			.53				
			.66				
			.50				
	.20		.52				26
			.39				
Retirement							
	.22					.24	
		.20					.31
Birth of children							

Table 6.4 (page 3 of 3)

	Involuntary Job Changes	Illness Hours	Disability	Involuntary Residential Move	Decrease in Family Income	Retirement
Illness hours						
	.29					
Disability						
		.31				
		.21				
Involuntary residential move						
Decrease in family income						
	.24					
Retirement		.22				
				.20	.25	
				.21	.20	
			.33			
Birth of children						
		.22				

Note: Table entries show correlations for each of the five age cohorts if their absolute value exceeds .2.
Key: .XX = correlation for those less than 30 years of age
.XX = correlation for those 30-39 years of age
.XX = correlation for those 40-49 years of age
.XX = correlation for those 50-59 years of age
.XX = correlation for those more than 59 years of age

DETERMINANTS AND CONSEQUENCES OF MAJOR LIFE EVENTS FOR MARRIED MEN

The correlates of life events and the effects of life events on two additional outcomes were explored with the causal model depicted in Figure 6.4. The causal flow runs from left to right, with arrows indicating the hypothesized causal paths. Life events play the role of intervening variables in this model, being determined by a set of background and attitudinal variables, and, in conjunction with those variables, determining changes in the two available attitudinal measures of efficacy and future orientation. The curved arrows between the measures of background and initial attitudes indicate that correlations among those measures are allowed but the causal ordering of them is not explored. The equations of the model can be written as follows:

(1) $\text{Life event}_j = f_j$ (Background, 1968 Efficacy, 1968 Future Orientation)

(2) Change in Efficacy = g (Life event_j, Background, 1968 Efficacy)

(3) Change in Future Orientation = h (Life event_j, Background, 1968 Future Orientation)

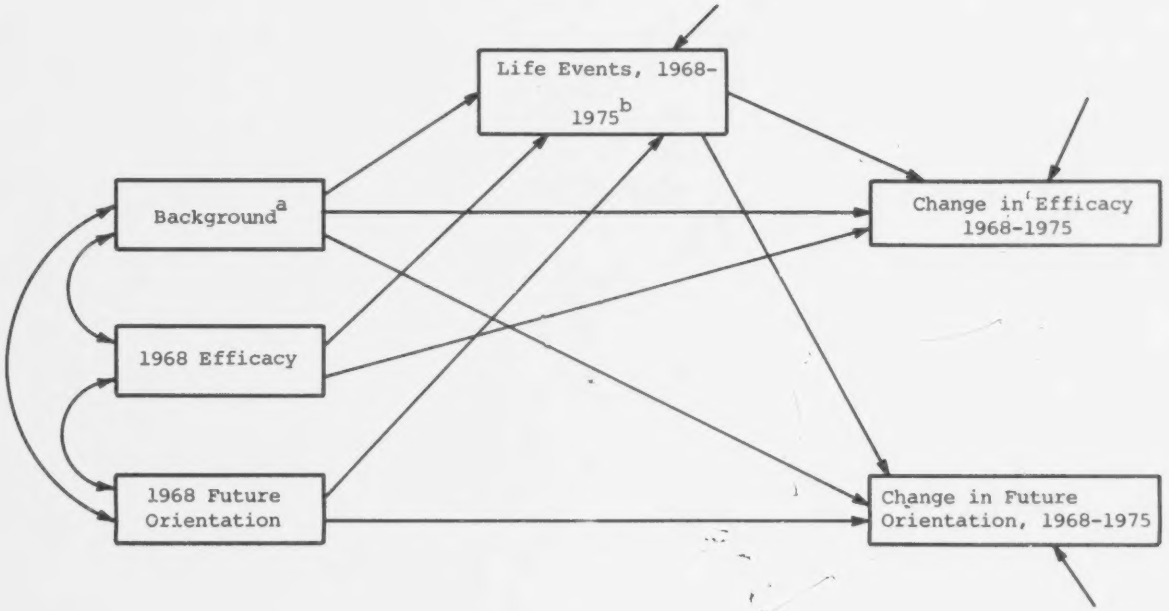
where "Background" consists of the following variables:

1) Education--years of educational attainment, reported in 1976.
2) Age--years of age in 1968.
3) Whether savings--A dichotomous variable equalling one if the respondent reported in 1969 having two months income saved, and equalling zero otherwise (The question was not asked in 1968).
4) Region--Three dichotomous variables indicating residence in the Northeast, North Central or Western regions, respectively. The Southern region is omitted, so coefficients on the included region variables indicate deviations from the Southern region.
5) Total family income--reported in 1968 for the calendar year 1967, in thousands of dollars.
6) Whether black--a dichotomous measure of race.
7) ln city size--the natural logarithm of city size in 1968.

"Life Events" consist of those events available for married male household heads in 1968, listed in Table 6.1. The model was also estimated separately for the indexes of desirable and undesirable events. "Changes in Efficacy and Future Orientation" are the absolute change in the three efficacy questions asked in both 1968 and 1975 and the absolute change in the future orientation question asked in both 1968 and 1975. Efficacy is the concept of a sense of personal effectiveness in carrying out plans, finishing things, and being certain that plans will be carried out. Future orientation reflects the extent to which

Figure 6.4

A CAUSAL MODEL OF BACKGROUND, LIFE EVENTS, AND CHANGE IN ATTITUDES



Life Events, 1968-1975[b]
Background[a]
1968 Efficacy
1968 Future Orientation
Change in Efficacy 1968-1975
Change in Future Orientation, 1968-1975

[a] (1) Education; (2) Age; (3) Whether Savings; (4) Region, (5) Total Family Income, (6) Whether Black; (7) ln City Size.

[b] (1) Remarriages; (2) Voluntary Residential Move; (3) Widowing; (4) Divorce; (5) Major Unemployment; (6) Involuntary Job Changes; (7) Becoming Disabled; (8) Involuntary Residential Move; (9) Decrease in Family Size; (10) Increase in Family Size; (11) Major Increase in Labor Income; (12) Major Increase in Work Hours; (14) Retirement.

respondents report planning ahead rather than living from day-to-day. The responses to these efficacy and future orientation questions were coded into three categories and equalled +1 if the respondent indicated strong positive response to efficacy or future orientation, -1 if the response was strongly negative, and 0 otherwise. (In all cases, more than three-quarters of the respondents gave strong positive or negative responses.) The change in efficacy variable ranges from -6 to +6. Change in future orientation ranges from -2 to +2.

To facilitate the estimation of the model, it was assumed that all the functional forms f_j, g and h are linear and, with one exception (age), additive, that the error term associated with each equation is independent of the variable in that equation, and that the error terms are independent of one another. To allow for possible interactions between age and the other variables, the model was estimated separately for the following five age cohorts: under 30 years of age in 1968, 30-39 in 1968, 40-49 in 1968, 50-59 in 1968, and older than 60 in 1968. The sample consists of married male household heads in 1968 who were respondents in both 1968 and 1975.[6]

Effects of Background in the Incidence of Life Events

The regression estimates of the effects of background variables on life events (Equation 1) are given in Tables 6.5 and 6.6. Rather than treat each individual event as a dependent variable, we use the indexes of desirable and undesirable events. For the most part, the background variables accounted for more of the variation in the undesirable event index than in the desirable event index.

Regarding desirable events, it can be seen that virtually none of the background variables have consistently significant effects across all five age cohorts. The declining incidence of desirable events across the age cohorts

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[6] Since the efficacy and future orientation questions have not been asked since 1975, we couldn't use the information on the life events that occurred between 1976 and 1978. Panel Study procedures dictate that the husband is the head of husband-wife families and that wives can be respondents only if the husband refuses to be. The number of observations available for this regression analysis is smaller than the number of observations available for the descriptive analysis of life events given in the appendix tables since we excluded instances where the male head was not the respondent. Note also that models with attitudinal variables measured after 1968 could not be estimated for women who headed families in 1968 since some of them married and their husbands became the respondents. Those who remained family heads would constitute an unrepresentative subset of all initially unmarried female heads.

Table 6.5

EFFECTS OF BACKGROUND VARIABLES ON TOTAL NUMBER OF DESIRABLE EVENTS, BY AGE
(For all married male household heads in 1968.)

	Age in 1968				
Independent Variables	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Education of head	.115* (.049)	-.042 (.031)	.009 (.022)	-.037 (.024)	-.003 (.025)
Age	-.275** (.045)	-.068* (.028)	-.015 (.023)	-.051* (.025)	-.027 (.018)
Whether savings	-.355 (.248)	-.533** (.180)	-.054 (.144)	-.098 (.156)	.014 (.196)
Northeast	-.773* (.341)	-.597* (.256)	-.304 (.204)	-.201 (.222)	-.161 (.243)
North central	-.419 (.275)	-.520* (.224)	-.265 (.188)	-.486** (.188)	.080 (.206)
West	-.093 (.361)	-.505+ (.284)	.169 (.223)	-.474* (.246)	-.031 (.250)
Total family income/1000	-.091* (.039)	.021 (.019)	-.010 (.011)	.002 (.012)	-.009 (.012)
Whether black	-.914+ (.483)	-.581+ (.351)	-.169 (.283)	-.093 (.259)	.282 (.324)
_ln_ city size	.236** (.068)	-.009 (.049)	-.018 (.038)	.038 (.040)	-.031 (.045)
Efficacy in 1968	-.057 (.068)	-.148* (.058)	.010 (.041)	-.155** (.048)	-.041 (.049)
Future orientation in 1968	-.177 (.140)	.282** (.101)	-.106 (.075)	-.066 (.080)	.004 (.097)
R^2 (adj.)	.219	.059	.008	.087	.028

Note: Standard errors are given in parentheses.
+ significant at .10 level
* significant at .05 level
**significant at .01 level

appeared to hold within the age cohorts as well, especially for the youngest one. The two indicators of economic well-being, savings and family income, had a few unexpected _negative_ effects on the number of desirable events, but that may be partly definitional since a doubling of family income was counted as one of the desirable events and that was rare for those with high initial incomes.

Table 6.6

EFFECTS OF BACKGROUND VARIABLES ON TOTAL NUMBER OF UNDESIRABLE EVENTS, BY AGE
(For all married male household heads in 1968.)

	Age in 1968				
Independent Variables	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Education of head	-.145**	-.115**	-.172**	-.140**	-.074*
	(.046)	(.034)	(.033)	(.049)	(.030)
Age	-.143**	-.036	.077*	.082+	.007
	(.042)	(.031)	(.035)	(.045)	(.021)
Whether savings	-.501*	-.097	-.328	-.487+	-.124
	(.234)	(.200)	(.214)	(.286)	(.230)
Northeast	.485	.662*	.490	.836*	.099
	(.322)	(.283)	(.303)	(.409)	(.286)
North central	.174	.383	.409	.138	.094
	(.260)	(.249)	(.280)	(.345)	(.242)
West	1.208**	.610+	.435	.045	-.114
	(.341)	(.315)	(.332)	(.452)	(.294)
Total family income/1000	-.035	-.017	-.023	.008	.024
	(.037)	(.021)	(.017)	(.023)	(.015)
Whether black	.670	.981*	-.234	-.369	.632+
	(.456)	(.391)	(.420)	(.470)	(.382)
ln city size	.066	.017	.034	.013	-.024
	(.064)	(.054)	(.057)	(.074)	(.053)
Efficacy in 1968	-.084	-.204**	-.059	-.200*	.104+
	(.065)	(.064)	(.061)	(.089)	(.058)
Future orientation in 1968	-.402**	-.173	-.185+	.121	.075
	(.132)	(.112)	(.112)	(.147)	(.115)
R^2 (adj.)	.201	.111	.135	.079	.043

Note: Standard errors are given in parentheses.
+ significant at .10 level
* significant at .05 level
**significant at .01 level

Virtually all of the region variables had negative coefficients and some of them were statistically significant. The omitted region is the South, so the negative coefficients indicate that those living in the South experienced more favorable events in the past ten years than those living elsewhere. Given the economic growth that has occurred there recently, this result is not surprising.

The remaining significant coefficients indicate that urban young people experienced more desirable events than the non-urban young, that initial efficacy had surprising negative effects on desirable events in two of the age cohorts, and that initial future orientation showed the expected positive effect only for the 30-39 group and showed insignificant negative effects elsewhere. All in all, these regressions suggest very few consistent effects of these background variables on the incidence of desirable life events.

Of greater interest are the determinants of the undesirable events. The background regressions indicate the extent to which initial economic well-being, attitudes, and demographic factors served to protect these married men from the undesirable events. The results in Table 6.6 show that educational attainment was a strong deterrent to such events, even when the effects of current income, savings, attitudes, and location have been taken into account. The size of the coefficients indicate that, on average, those with a college degree experienced one less undesirable event than those with only an eighth grade education. Furthermore, these education effects did not decline with age--a fact which runs contrary to the view that education has a short-term effect as a kind of job getting credential. Our finding fits in with considerable evidence that education leads to more planning and advanced problem-solving as well as stabler jobs. But those who got more education also started with backgrounds and personalities that differed in many measured and unmeasured ways.

Assets were more important in preventing undesirable events than income, as indicated by the consistently negative and often significant coefficients on the savings variable and the completely insignificant effects of the income variable. It is unclear, however, whether the assets themselves helped to prevent the undesirable events (e.g., purchases of preventive medicine preventing disability; high standard of living preventing divorce) or whether those who saved had other, unmeasured characteristics which helped them to avoid the undesirable events.

Several of the region variables were significant in these regressions also. The positive coefficients here indicate that those living in the South were less likely to experience undesirable events--a result that complements the findings that they also experienced more desirable events.

The efficacy and future orientation variables often showed significant negative effects on bad events, indicating that those with more initial efficacy and orientation toward the future were less likely to experience as many undesirable events. Higher initial scores on the future orientation index benefitted the very youngest group of married men the most, while higher efficacy scores were most effective for men in their 30s and 50s.

The Effects of Events on Changes in Efficacy and Future Orientation

The Panel Study data do not contain many of the commonly discussed or studied consequences of life events. We have little information about the physical or mental health of our respondents. Repeated measurement of efficacy and future orientation does however provide information on change in two potentially important psychological states, and these two variables were included in the model as outcome variables. Estimating Equations 2 and 3 for the effects of the indexes of desirable and undesirable events gives an aggregate look at the effects of those life events on changes in efficacy and in future orientation.[7] The results, shown in Table 6.7, are easily summarized: Undesirable events had consistent negative effects on people's sense of personal efficacy, especially for the very young and very old. Taken together, desirable events had no significant relationship to changes in either one of the attitudinal measures, and undesirable events had no consistent effects on changes in future orientation. The combined results of Tables 6.5 and 6.7 provide some evidence for a dynamic-feedback model between efficacy and undesirable events since lower initial efficacy was found to be positively related to subsequent undesirable events, which in turn were found to produce a drop in reported efficacy. The

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[7] The form of the equations which relate changes in efficacy and future orientation to the events and background variables was chosen with care. The dependent variables are the absolute changes in each of the attitudes and the initial levels of the attitudes are included on the right hand side. Thus, we have

(4) $1976A - 1968A = a + b1968A + cX$

where A is the attitudinal measure and X is an independent variable such as education or an event measure.

Since the initial level of an attitude is negatively correlated with the absolute change in that attitude, the inclusion of the initial attitude on the right hand side of the equation affects the size and interpretation of the other coefficients. The "c" coefficient does not simply measure the effect of a unit change in X on change in attitudes. This can be seen if (4) is rewritten as:

(5) $1976A - (1 + b)\ 1968A = a + cX$

The interpretation of c depends upon the size of the b coefficient. We would argue that the initial attitude belongs on the right hand side because those who begin with high scores on the A variable cannot have positive changes in A, while those who begin with low scores on A cannot have negative changes. Inclusion of the initial A variable helps to adjust for these effects. As a final note, it can be shown that the "c" coefficients obtained by estimating equation (4) are identical to those obtained from estimating a change equation with 1976A as the dependent variable and 1968A as an independent variable.

Table 6.7

EFFECTS OF INDEXES OF DESIRABLE AND UNDESIRABLE EVENTS ON CHANGE IN EFFICACY AND FUTURE ORIENTATION

(For all married male household heads in 1968.)

	Age in 1968				
	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Dependent Variable: Change in Efficacy					
Independent Variable:					
Index of Desirable Events	.006 (.040)	-.015 (.044)	-.054 (.057)	-.021 (.080)	-.094 (.115)
Index of Undesirable Events	-.156** (.042)	-.075+ (.040)	-.066+ (.039)	.009 (.044)	-.200* (.098)
R^2 (adj.)	.391	.358	.391	.402	.374
Dependent Variable: Change in Future Orientation					
Independent Variable:					
Index of Desirable Events	.020 (.023)	-.007 (.025)	.073* (.035)	.021 (.048)	-.066 (.072)
Index of Undesirable Events	.008 (.024)	-.011 (.023)	-.029 (.023)	-.017 (.026)	.008 (.061)
R^2 (adj.)	.257	.276	.323	.298	.283

Note: All regressions include background variables and initial attitudes. Standard errors are given in parentheses.
+ Significant at .10 level
* Significant at .05 level
**Significant at .01 level

possible existence of such a model is intriguing, but the variability of the relevant coefficients in Tables 6.5 and 6.7 cautions against over-confident interpretation.

Since indexes may mask the effects of the individual events, we also estimated Equations 2 and 3 with most of the individual event variables. Events excluded from these regressions had consistently insignificant effects in preliminary regression analysis.[8] Extremely rare events were excluded from the regressions for some of the age cohorts (e.g., becoming widowed for those less than 40 years of age).

Regression results on the effects of events on changes in efficacy and future orientation variables are shown in Tables 6.8 and 6.9, respectively. Although none of the events had significant effects across all five age cohorts, several are worthy of note:

1) Disability lowered feelings of efficacy and future orientation in all instances, although the estimated coefficients were statistically significant at conventional levels only for the middle three age cohorts for efficacy and for the two oldest age cohorts for future orientation. Such a rare event has to have a substantial effect to achieve statistical significance.
2) Increases but not decreases in family size usually had negative and significant effects on changes in both efficacy and future orientation. The negative effects were most consistent for the future orientation measure. Interestingly, most of the coefficients changed very little when the "Birth of Children" measure was added, indicating that other additions to the family (such as the parents, grandchildren, or stepchildren of the household head) were causing this effect. It is easy to imagine, for example, how the addition of a disabled parent to a household might cause such changes.
3) Involuntary residential moves lowered efficacy scores, particularly for those married men who were in their 40s at the beginning of the period.
4) Labor market events appeared to have the greatest effect on the feeling of personal control for those in the youngest age cohort. Large increases in labor income were associated with increases in reported efficacy while large decreases had negative effects. The coefficients

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[8] It may seem that several of the included events (e.g., increases in work hours and increases in labor income) are highly intercorrelated and might cause multicolinearity problems. We found that this was not the case, however.

Table 6.8 (page 1 of 2)

EFFECTS OF EVENTS ON CHANGES IN EFFICACY
(For all married male household heads in 1968.)

	Age				
Independent Variables	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Number of remarriages	.233 (.314)	.840* (.400)	-.017 (.320)	-.010 (.943)	-.735 (.812)
Voluntary job changes	.001 (.079)	.111 (.103)	-.238* (.117)	.178 (.195)	.421 (.385)
Becoming Widowed	a	a	-.716 (.512)	.090 (.469)	-.261 (.393)
Number of Divorces	.218 (.313)	-.338 (.362)	.163 (.441)	-.104 (.635)	-.788 (.559)
Number of times unemployed	-.142 (.094)	-.215** (.080)	.039 (.074)	-.100 (.097)	-.529+ (.289)
Involuntary job change	-.194 (.133)	.191 (.131)	-.094 (.143)	.232 (.186)	a
Disability	-.279 (.212)	-.430* (.198)	-.357* (.175)	-.424* (.178)	-.339 (.212)
Involuntary residential move	-.184 (.136)	-.514* (.226)	-1.172** (.369)	-.101 (.325)	-.516 (.723)

on virtually all of the other job-related variables were in the expected directions and exceeded their standard errors.

5) Change in marital status had surprisingly weak effects on both of the attitudinal measures. None of the coefficients for becoming widowed were statistically significant and only one of the "remarriage" coefficients were significant. Divorce increased future orientation, significantly so for men in their 30s and beyond the age of 60.

Summary

The Panel Study data contain numerous measures of life events for various groups of sample individuals. The most detailed data are available for the men who began the panel period (1968-1978) as married heads of households. For this group we found the following:

1) Virtually all of them experienced at least one of the events, two-thirds of them experienced at least one undesirable event.

Table 6.8 (page 2 of 2)

	Age				
Independent Variables	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Decrease in family size	-.206 (.152)	-.098 (.097)	-.007 (.068)	.073 (.108)	-.093 (.191)
Increase in family size	-.252** (.098)	-.236* (.103)	.207+ (.114)	-.257+ (.155)	.103 (.200)
Increase in labor income	.148* (.069)	-.023 (.072)	.114 (.081)	.124 (.128)	-.169 (.251)
Decrease in labor income	-.367** (.135)	-.136 (.130)	-.161 (.146)	.172 (.115)	.084 (.150)
Increase in hours	.110 (.091	.207* (.092)	.056 (.103)	-.134 (.133)	.193 (.273)
Retirement	[a]	[a]	-.343 (.303)	.408 (.217)	.303 (.222)
R^2 (adj.)	.411	.387	.418	.422	.363

Note: All regressions include background variables and initial attitudes. Standard errors are given in parentheses.

[a] Event was too rare in the age cohort to be included in the model.

+ Significant at .10 level

* Significant at .05 level

**Significant at .01 level

2) Taken together, undesirable events were independent of age. Some (involuntary job and residential changes) were more frequent among the young, while others (disability, widowhood) were more common at older ages.

3) The demographic characteristics of the married men explained differences in the incidence of undesirable events much better than differences in the incidence of desirable events. Educational attainment was an important deterrent to undesirable events and its effect did not fall with age. Savings helped to prevent undesirable events, as did living in the South and having high scores on the efficacy and future orientation indexes. Taken together, undesirable events were associated with significant negative changes in reported efficacy.

Table 6.9 (page 1 of 2)

EFFECTS OF EVENTS ON CHANGES IN FUTURE ORIENTATION
(For all married female household heads in 1968.)

	Age				
Independent Variables	Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Number of remarriages	.245 (.178)	-.294 (.230)	.171 (.316)	.525 (.557	.115 (.493)
Voluntary job changes	.000 (.044)	.012 (.059)	.009 (.072)	.036 (.116)	.128 (.238)
Becoming Widowed	a	a	.261 (.311)	-.374 (.277)	-.146 (.241)
Number of Divorces	.114 (.177)	.493* (.206)	.043 (.267)	.330 (.379)	.840* (.342)
Number of times unemployed	-.064 (.053)	.049 (.046)	-.055 (.045)	.035 (.058)	.142 (.178)
Involuntary job change	.059 (.075)	-.176* (.076)	-.043 (.087)	-.098 (.111)	a
Disability	-.004 (.119)	-.084 (.114)	-.081 (.104)	-.229* (.105)	-.252+ (.130)

4) Several of the individual life events showed consistent effects on changes in efficacy and future orientation. Disability and increases in family size had substantial negative effects on both.

The pattern of life events for the other groups of sample individuals were usually similar to the patterns for married men. Married women differed somewhat in that for them the incidence of undesirable events tended to increase with age.

Table 6.9 (page 2 of 2)

Independent Variables	Age Less than 30 Years	30-39 Years	40-49 Years	50-59 Years	More than 59 Years
Involuntary residential move	.033 (.076)	-.064 (.129)	-.212 (.224)	.265 (.193)	.188 (.442)
Decrease in family size	.133 (.085)	-.056 (.055)	.047 (.041)	.004 (.064)	.124 (.117)
Increase in family size	-.112* (.055)	-.083 (.060)	-.171* (.069)	-.159+ (.092)	-.195 (.121)
Increase in labor income	.006 (.038)	.012 (.042)	.005 (.049)	-.204** (.076)	.226 (.153)
Decrease in labor income	-.044 (.075)	-.006 (.074)	.034 (.088)	.058 (.069)	-.093 (.093)
Increase in hours	.062 (.051)	.038 (.053)	.238** (.062)	.106 (.079)	-.028 (.167)
Retirement	a	a	.268 (.184)	-.117 (.129)	-.084 (.137)
R^2 (adj.)	.278	.281	.339	.337	.304

Note: All regressions include background variables and initial attitudes. Standard errors are given in parentheses.

[a] Event was too rare in the age cohort to be included in the model.

+ Significant at .10 level
* Significant at .05 level
**Significant at .01 level

References

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Table A6.1

FREQUENCY OF (RE)MARRIAGE BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of (Re)marriages						Range			Weighted
Age in 1968	0	1	2	3	Total	Mean	Minimum	Maximum	Number of Observations	Percentage of Observations
Less than 30 years	83.3%	15.5%	0.8%	0.4%	100.0%	0.183	0	3	420	19.4%
30-39 years	90.5	8.8	0.7	0.0	100.0	0.102	0	2	531	24.1
40-49 years	93.3	6.2	0.5	0.0	100.0	0.072	0	2	520	25.2
50-59 years	96.4	3.6	0.0	0.0	100.0	0.036	0	1	335	16.4
60-64 years	95.2	4.8	0.0	0.0	100.0	0.048	0	1	100	7.3
65-69 years	95.9	4.1	0.0	0.0	100.0	0.041	0	1	64	4.6
More than 69 years	97.7	2.3	0.0	0.0	100.0	0.023	0	1	42	3.0
All	91.6%	7.9%	0.4%	0.1%	100.0%	0.090	0	3	2012	100.0%

TauB= -0.1382

Gamma= -0.3946

Table A6.2

FREQUENCY OF VOLUNTARY JOB CHANGES BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Voluntary Job Changes								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	31.2%	32.6%	17.9%	8.2%	6.2%	4.0%	100.0%	1.400	0	8	420	19.4%
30-39 years	56.5	26.8	10.4	5.0	0.8	0.5	100.0	0.682	0	5	531	24.1
40-49 years	70.2	18.1	8.8	2.4	0.6	0.0	100.0	0.450	0	4	520	25.2
50-59 years	78.5	18.7	1.9	0.8	0.0	0.1	100.0	0.252	0	5	335	16.4
60-64 years	88.2	9.5	2.3	0.0	0.0	0.0	100.0	0.141	0	2	100	7.3
65-69 years	96.0	4.0	0.0	0.0	0.0	0.0	100.0	0.040	0	1	64	4.6
More than 69 years	97.7	0.0	2.3	0.0	0.0	0.0	100.0	0.047	0	2	42	3.0
All	64.0%	21.3%	8.8%	3.5%	1.5%	0.9%	100.0%	.605	0	8	2012	100.0%

TauB= -0.3608
Gamma= -0.5479

Table A6.3

FREQUENCY OF MAJOR INCREASE IN FAMILY INCOME BY AGE, 1968-1978

(For all married male household heads in 1968.)

Age in 1968	Number of Major Increases in Family Income						Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3	4	Total		Minimum	Maximum		
Less than 30 years	71.4%	21.2%	5.6%	1.4%	0.4%	100.0%	0.380	0	4	420	19.4%
30-39 years	78.3	15.7	4.2	1.3	0.5	100.0	0.299	0	4	531	24.1
40-49 years	82.2	13.9	2.7	1.1	0.0	100.0	0.228	0	4	520	25.2
0-59 years	77.4	16.1	4.2	2.0	0.4	100.0	0.318	0	4	335	16.4
60-64 years	75.5	17.4	5.1	2.0	0.0	100.0	0.336	0	3	100	7.3
65-69 years	77.8	16.0	4.7	1.5	0.0	100.0	0.298	0	3	64	4.6
More than 69 years	60.6	37.1	2.3	0.0	0.0	100.0	0.418	0	2	42	3.0
All	77.1%	17.1%	4.1%	1.4%	0.3%	100.0%	0.306	0	4	2012	100.0%

TauB= -0.0168
Gamma= -0.0303

Table A6.4

FREQUENCY OF VOLUNTARY RESIDENTIAL MOVES BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Voluntary Residential Moves: 0	1	2	3	4	5	6	7 or More	Total	Mean	Range: Mini-mum	Range: Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	15.1%	29.2%	20.5%	14.4%	9.7%	4.7%	4.4%	1.9%	100.0%	2.169	0	9	420	19.4%
30-39 years	45.6	24.4	10.9	11.3	5.7	0.5	1.1	0.4	100.0	1.159	0	8	531	24.1
40-49 years	62.3	24.0	5.9	3.5	2.8	0.7	0.6	0.1	100.0	0.656	0	7	520	25.2
50-59 years	68.0	19.5	8.4	2.3	1.4	0.1	0.5	0.0	100.0	0.516	0	6	335	16.4
60-64 years	62.5	29.4	6.7	0.0	0.5	1.0	0.0	0.0	100.0	0.494	0	5	100	7.3
65-69 years	69.7	13.6	9.7	4.1	1.5	0.0	1.4	0.0	100.0	0.600	0	6	64	4.6
More than 69 years	79.0	21.0	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.210	0	1	42	3.0
All	50.9%	24.2%	10.4%	7.0%	4.3%	1.3%	1.4%	0.5%	100.0%	1.021	0	9	2012	100.0%

TauB= -0.3353
Gamma= -0.4537

Table A6.5

FREQUENCY OF BECOMING WIDOWED BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Times Individuals Become Widowed				Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	Total		Minimum	Maxi-mum		
Less than 30 years	100.0%	0.0%	0.0%	100.0%	0.000	0	1	420	19.4%
30-39 years	98.1	1.9	0.0	100.0	0.019	0	1	531	24.1
40-49 years	96.1	3.9	0.0	100.0	0.040	0	2	520	25.2
50-59 years	93.9	6.1	0.0	100.0	0.061	0	1	335	16.4
60-64 years	88.3	11.7	0.0	100.0	0.117	0	1	100	7.3
65-69 years	79.7	20.3	0.0	100.0	0.203	0	1	64	4.6
More than 69 years	74.7	25.3	0.0	100.0	0.253	0	1	42	3.0
All	95.0%	5.0%	0.0%	100.0%	0.050	0	2	2012	100.0%

TauB= 0.1961
Gamma= 0.6558

Table A6.6

FREQUENCY OF DIVORCE OR SEPARATION BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Divorces or Separations						Range			
Age in 1968	0	1	2	3	Total	Mean	Mini-mum	Maxi-mum	Number of Observations	Weighted Percentage of Observations
Less than 30 years	82.2%	15.5%	1.7%	0.6%	100.0%	0.208	0	3	420	19.4%
30-39 years	88.9	10.7	0.4	0.0	100.0	0.114	0	3	531	24.1
40-49 years	94.3	4.6	1.1	0.0	100.0	0.067	0	2	520	25.2
50-59 years	98.1	1.4	0.5	0.0	100.0	0.023	0	2	335	16.4
60-64 years	97.6	2.4	0.0	0.0	100.0	0.024	0	1	100	7.3
65-69 years	92.9	7.1	0.0	0.0	100.0	0.071	0	1	64	4.6
More than 69 years	90.8	9.2	0.0	0.0	100.0	0.092	0	1	42	3.0
All	91.4%	7.8%	0.8%	0.1%	100.0%	0.097	0	3	2012	100.0%

TauB= -0.1535
Gamma= -0.4297

Table A6.7

FREQUENCY OF MAJOR UNEMPLOYMENT BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Periods of Major Unemployment: 0	1	2	3	4	5	6	7	8	9 or More	Total	Mean	Range: Minimum	Range: Maximum	Number of Observations	Weighted Percentage of Observations
Less than 30 years	63.5%	13.9%	10.2%	6.0%	3.5%	1.0%	0.9%	1.1%	0.0%	0.0%	100.0%	0.842	0	10	420	19.4%
30-39 years	62.9	16.5	9.1	5.2	1.4	1.3	1.7	1.3	0.5	0.0	100.0	0.865	0	11	531	24.1
40-49 years	67.7	13.5	6.6	4.8	1.7	2.3	1.7	0.1	1.1	0.6	100.0	0.854	0	11	520	25.2
50-59 years	75.8	13.9	5.2	1.7	0.7	2.3	0.1	0.1	0.0	0.2	100.0	0.466	0	9	335	16.4
60-64 years	90.8	7.2	1.0	0.0	0.0	1.0	0.0	0.0	0.0	0.0	100.0	0.144	0	5	100	7.3
65-69 years	91.9	5.7	1.2	1.2	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.117	0	3	64	4.6
More than 69 years	95.1	2.6	2.3	0.0	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.072	0	2	42	3.0
All	70.6%	13.2%	6.9%	4.0%	1.6%	1.6%	1.0%	0.6%	0.4%	0.2%	100.0%	0.682	0	11	2012	100.0%

TauB= -0.1659
Gamma= -0.2723

Table A6.8

FREQUENCY OF INVOLUNTARY JOB CHANGES BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Involuntary Job Changes								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	66.5%	16.9%	12.0%	3.0%	1.1%	0.4%	100.0%	0.568	0	7	420	19.4%
30-39 years	70.8	16.4	9.6	2.1	0.9	0.2	100.0	0.472	0	7	531	24.1
40-49 years	77.5	13.2	5.3	1.7	1.3	1.1	100.0	0.395	0	6	520	25.2
50-59 years	86.3	9.0	2.6	2.1	0.0	0.0	100.0	0.206	0	3	335	16.4
60-64 years	94.5	3.6	1.9	0.0	0.0	0.0	100.0	0.074	0	2	100	7.3
65-69 years	98.6	1.4	0.0	0.0	0.0	0.0	100.0	0.014	0	1	64	4.6
More than 69 years	100.0	0.0	0.0	0.0	0.0	0.0	100.0	0.000	0	0	42	3.0
All	78.0%	12.4%	6.5%	1.9%	0.8%	0.5%	100.0%	0.363	0	7	2012	100.0%

TauB= -0.2074
Gamma= -0.3865

Table A6.9

FREQUENCY OF MAJOR WORK LOSS DUE TO ILLNESS BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Years with Major Work Loss Due to Illness										Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3	4	5	6	7 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	78.5%	13.1%	5.5%	2.1%	0.8%	0.0%	0.0%	0.0%	100.0%	0.335	0	5	420	19.4%
30-39 years	72.5	15.2	8.3	2.7	0.3	0.1	0.2	0.7	100.0	0.485	0	8	531	24.1
40-49 years	61.8	18.0	12.6	4.3	1.2	0.6	0.3	1.1	100.0	0.750	0	9	520	25.2
50-59 years	64.7	24.8	5.5	2.1	2.1	0.5	0.3	0.0	100.0	0.545	0	6	335	16.4
60-64 years	81.4	12.8	3.8	0.9	1.0	0.0	0.0	0.1	100.0	0.278	0	7	100	7.3
65-69 years	92.1	7.9	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.079	0	1	64	4.6
More than 69 years	97.4	2.6	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.026	0	1	42	3.0
All	72.0%	16.2%	7.5%	2.5%	0.9%	0.3%	0.2%	0.5%	100.0%	0.485	0	9	2012	100.0%

TauB= -0.0072
Gamma= -0.0122

Table A6.10

FREQUENCY OF BECOMING DISABLED BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Times Individuals Became Disabled						Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3	4	Total		Minimum	Maximum		
Less than 30 years	82.9%	14.3%	2.6%	0.2%	0.0%	100.0%	0.202	0	3	420	19.4%
30-39 years	82.8	14.0	2.8	0.4	0.0	100.0	0.209	0	3	531	24.1
40-49 years	73.1	19.5	6.5	0.8	0.0	100.0	0.350	0	3	520	25.2
50-59 years	55.6	28.4	13.8	1.9	0.2	100.0	0.628	0	4	335	16.4
60-64 years	57.0	23.7	17.6	1.7	0.0	100.0	0.640	0	3	100	7.3
65-69 years	32.3	38.9	27.2	1.6	0.0	100.0	0.982	0	3	64	4.6
More than 69 years	15.6	49.2	28.4	6.9	0.0	100.0	1.265	0	3	42	3.0
All	69.7%	20.7%	8.5%	1.1%	0.0%	100.0%	0.410	0	4	2012	100.0%

TauB= 0.2840
Gamma= 0.4476

Table A6.11

FREQUENCY OF INVOLUNTARY RESIDENTIAL MOVES BY AGE, 1968-1978

(For all married male household heads in 1968.)

	Number of Involuntary Residential Moves								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5	Total	Mean	Minimum	Maximum		
Less than 30 years	75.0%	16.7%	5.5%	1.8%	0.6%	0.4%	100.0%	0.374	0	5	420	19.4%
30-39 years	87.6	9.4	2.5	0.4	0.0	0.0	100.0	0.159	0	4	531	24.1
40-49 years	91.6	6.7	1.4	0.2	0.0	0.0	100.0	0.103	0	3	520	25.2
50-59 years	87.9	9.4	2.6	0.1	0.0	0.0	100.0	0.148	0	3	335	16.4
60-64 years	93.0	7.0	0.0	0.0	0.0	0.0	100.0	0.070	0	1	100	7.3
65-69 years	94.0	6.0	0.0	0.0	0.0	0.0	100.0	0.060	0	1	64	4.6
More than 69 years	86.1	13.9	0.0	0.0	0.0	0.0	100.0	0.139	0	1	42	3.0
All	86.9%	9.9%	2.4%	0.5%	0.1%	0.1%	100.0%	0.173	0	5	2012	100.0%

TauB= -0.1261
Gamma= -0.2892

Table A6.12

FREQUENCY OF MAJOR DECREASES IN FAMILY INCOME BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Major Decreases in Income								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	85.3%	11.1%	2.3%	1.0%	0.0%	0.2%	100.0%	0.198	0	5	420	19.4%
30-39 years	86.9	11.2	1.7	0.2	0.0	0.0	100.0	0.153	0	4	531	24.1
40-49 years	86.6	10.8	2.0	0.5	0.0	0.0	100.0	0.164	0	3	520	25.2
50-59 years	79.9	15.9	3.1	1.2	0.0	0.0	100.0	0.255	0	3	335	16.4
60-64 years	72.7	25.3	2.0	0.0	0.0	0.0	100.0	0.292	0	2	100	7.3
65-69 years	83.9	14.6	1.5	0.0	0.0	0.0	100.0	0.176	0	2	64	4.6
More than 69 years	74.6	20.8	4.6	0.0	0.0	0.0	100.0	0.300	0	2	42	3.0
All	83.9%	13.3%	2.2%	0.6%	0.0%	0.0%	100.0%	0.197	0	5	2012	100.0%

TauB= 0.0679
Gamma= 0.1410

Table A6.13

FREQUENCY OF DECREASES IN FAMILY SIZE BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Decreases in Family Size 0	1	2	3	4	5	6 or More	Total	Mean	Range Mini-mum	Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	59.5%	29.9%	7.9%	2.0%	0.7%	0.0%	0.0%	100.0%	0.545	0	5	420	19.4%
30-39 years	34.6	30.6	21.9	10.1	2.7	0.1	0.0	100.0	1.159	0	6	531	24.1
40-49 years	16.7	25.3	29.0	16.7	7.1	3.2	1.9	100.0	1.900	0	8	520	25.2
50-59 years	41.3	30.0	18.4	7.1	2.3	0.6	0.2	100.0	1.015	0	6	335	16.4
60-64 years	64.1	19.4	7.1	8.4	0.9	0.1	0.0	100.0	0.628	0	5	100	7.3
65-69 years	53.6	30.7	14.5	1.2	0.0	0.0	0.0	100.0	0.633	0	3	64	4.6
More than 69 years	59.4	31.9	4.8	1.9	2.1	0.0	0.0	100.0	0.553	0	4	42	3.0
All	39.8%	28.3%	18.5%	8.9%	3.1%	0.9%	0.5%	100.0%	1.122	0	8	2012	100.0%

TauB= 0.0404
Gamma= 0.0528

Table A6.14

FREQUENCY OF INCREASES IN FAMILY SIZE BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Increases in Family Size								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5	Total	Mean	Minimum	Maximum		
Less than 30 years	18.5%	32.4%	30.4%	12.9%	5.1%	0.7%	100.0%	1.557	0	5	420	19.4%
30-39 years	48.4	32.9	11.5	4.9	1.8	0.5	100.0	0.802	0	5	531	24.1
40-49 years	53.7	31.2	9.8	4.3	0.9	0.0	100.0	0.677	0	5	520	25.2
50-59 years	69.3	21.4	7.2	0.9	0.7	0.5	100.0	0.438	0	5	335	16.4
60-64 years	75.6	15.6	6.1	0.1	2.5	0.0	100.0	0.384	0	4	100	7.3
65-69 years	78.8	16.0	2.4	1.3	1.5	0.0	100.0	0.306	0	4	64	4.6
More than 69 years	80.3	17.9	0.0	0.0	1.9	0.0	100.0	0.253	0	4	42	3.0
All	51.7%	28.0%	12.9%	5.0%	2.1%	0.3%	100.0%	0.788	0	5	2012	100.0%

TauB= -0.3353
Gamma= -0.4653

Table A6.15

FREQUENCY OF MAJOR INCREASES IN LABOR INCOME BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Major Increases in Labor Income						Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	68.9%	23.6%	7.0%	0.5%	100.0%	0.391	0	3	420	19.4%
30-39 years	76.4	18.8	3.8	1.0	100.0	0.295	0	3	531	24.1
40-49 years	81.3	14.6	3.7	0.3	100.0	0.230	0	3	520	25.2
50-59 years	80.7	15.3	3.5	0.4	100.0	0.236	0	3	335	16.4
60-64 years	86.8	11.0	1.1	1.0	100.0	0.163	0	3	100	7.3
65-69 years	95.8	2.8	0.0	1.4	100.0	0.085	0	4	64	4.6
More than 69 years	95.2	4.8	0.0	0.0	100.0	0.048	0	1	42	3.0
All	79.1%	16.4%	3.9%	0.7%	100.0%	0.261	0	4	2012	100.0%

TauB= -0.1392
Gamma= -0.2662

Table A6.16

FREQUENCY OF MAJOR DECREASES IN LABOR INCOME BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Major Decreases in Labor Income					Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3 or More	Total		Mini-mum	Maxi-mum		
Less than 30 years	75.2%	17.2%	5.4%	.3%	100.0%	0.347	0	3	420	19.4%
30-39 years	73.7	19.5	5.7	1.1	100.0	0.343	0	3	531	24.1
40-49 years	75.2	18.1	5.4	1.3	100.0	0.330	0	4	520	25.2
50-59 years	38.8	36.8	19.5	4.9	100.0	0.918	0	5	335	16.4
60-64 years	13.5	42.1	30.3	14.1	100.0	1.478	0	5	100	7.3
65-69 years	55.4	33.1	10.1	1.3	100.0	0.573	0	3	64	4.6
More than 69 years	65.1	28.1	2.3	4.4	100.0	0.461	0	3	42	3.0
All	63.2%	24.0%	9.7%	3.0%	100.0%	0.531	0	5	2012	100.0%

TauB= 0.2467
Gamma= 0.3634

Table A6.17

FREQUENCY OF MAJOR INCREASES IN WORK HOURS BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Major Increases in Work Hours								Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4	5 or More	Total	Mean	Minimum	Maximum		
Less than 30 years	26.6%	30.8%	21.2%	14.3%	5.4%	1.6%	100.0%	1.463	0	6	420	19.4%
30-39 years	33.0	30.1	22.6	11.8	2.6	0.0	100.0	1.210	0	4	531	24.1
40-49 years	44.9	32.8	15.7	5.2	1.4	0.0	100.0	0.854	0	4	520	25.2
50-59 years	51.4	29.9	15.4	2.5	0.8	0.0	100.0	0.713	0	4	335	16.4
60-64 years	79.8	12.9	6.2	1.1	0.0	0.0	100.0	0.285	0	3	100	7.3
65-69 years	89.9	8.7	0.0	1.4	0.0	0.0	100.0	0.130	0	3	64	4.6
More than 69 years	88.7	11.3	0.0	0.0	0.0	0.0	100.0	0.113	0	1	42	3.0
All	45.5%	28.1%	16.5%	7.5%	2.2%	0.3%	100.0%	0.938	0	6	2012	100.0%

TauB= -0.3108
Gamma= -0.4199

Table A6.18

FREQUENCY OF MAJOR DECREASES IN WORK HOURS BY AGE, 1968-1978

(For all married male household heads in 1968.)

Age in 1968	Number of Major Decreases in Work Hours 0	1	2	3	4	5	Total	Mean	Range Mini-mum	Range Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	30.1%	33.8%	20.6%	10.1%	3.6%	1.8%	100.0%	1.286	0	5	420	19.4%
30-39 years	30.4	30.2	23.3	10.7	4.1	1.4	100.0	1.323	0	5	531	24.1
40-49 years	38.9	32.2	14.7	10.2	3.7	0.3	100.0	1.084	0	5	520	25.2
50-59 years	44.5	29.6	17.6	6.6	1.4	0.3	100.0	0.917	0	5	335	16.4
60-64 years	66.1	22.8	10.1	0.9	0.0	0.0	100.0	0.458	0	3	100	7.3
65-69 years	90.6	6.1	3.3	0.0	0.0	0.0	100.0	0.127	0	2	64	4.6
More than 69 years	88.8	8.9	0.0	2.3	0.0	0.0	100.0	0.158	0	3	42	3.0
All	41.9%	29.0%	17.1%	8.3%	2.8%	0.8%	100.0%	1.036	0	5	2012	100.0%

TauB= -0.2312
Gamma= -0.3080

Table A6.19

FREQUENCY OF RETIREMENT BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Retirements						Range			
Age in 1968	0	1	2	3	Total	Mean	Mini-mum	Maxi-mum	Number of Observations	Weighted Percentage of Observations
Less than 30 years	98.8%	1.0%	0.2%	0.0%	100.0%	0.014	0	2	420	19.4%
30-39 years	96.1	3.4	0.5	0.0	100.0	0.043	0	2	531	24.1
40-49 years	91.5	8.0	0.5	0.0	100.0	0.091	0	2	520	25.2
50-59 years	42.4	54.0	3.2	0.4	100.0	0.616	0	3	335	16.4
60-64 years	15.8	69.0	11.1	4.1	100.0	1.034	0	3	100	7.3
65-69 years	61.5	23.1	11.2	4.2	100.0	0.581	0	3	64	4.6
More than 69 years	79.3	16.3	4.4	0.0	100.0	0.251	0	2	42	3.0
All	78.7%	18.4%	2.3%	0.6%	100.0%	0.246	0	3	2012	100.0%

TauB= 0.4529
Gamma= 0.7656

Table A6.20

FREQUENCY OF BIRTH OF CHILDREN BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Children Born							Range			
Age in 1968	0	1	2	3	4 or More	Total	Mean	Mini-mum	Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	32.4%	34.7%	25.7%	6.1%	1.1%	100.0%	1.089	0	4	420	19.4%
30-39 years	70.1	25.0	3.4	0.9	0.6	100.0	0.370	0	4	531	24.1
40-49 years	92.9	6.2	0.8	0.0	0.0	100.0	0.080	0	5	520	25.2
50-59 years	97.7	1.5	0.8	0.0	0.0	100.0	0.031	0	2	335	16.4
60-64 years	98.8	1.2	0.0	0.0	0.0	100.0	0.012	0	1	100	7.3
65-69 years	98.5	1.5	0.0	0.0	0.0	100.0	0.015	0	1	64	4.6
More than 69 years	100.0	0.0	0.0	0.0	0.0	100.0	0.000	0	0	42	3.0
All	77.3%	14.8%	6.1%	1.4%	0.4%	100.0%	0.327	0	5	2012	100.0%

TauB= -0.4766
Gamma= -0.8356

Table A6.21

FREQUENCY OF ANY JOB CHANGE BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Job Changes 0	1	2	3	4	5	6	7	8 or More	Total	Mean	Range Mini-mum	Range Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	14.3%	17.8%	15.4%	16.7%	13.1%	10.5%	7.6%	2.0%	2.7%	100.0%	2.870	0	11	420	19.4%
30-39 years	37.2	18.5	17.9	11.8	6.2	4.5	1.9	1.1	0.9	100.0	1.634	0	8	531	24.1
40-49 years	51.1	17.7	14.7	7.7	4.0	1.9	1.8	0.9	0.2	100.0	1.144	0	9	520	25.2
50-59 years	64.4	19.4	10.1	3.3	2.7	0.1	0.0	0.0	0.1	100.0	0.611	0	8	335	16.4
60-64 years	79.0	16.0	3.7	1.3	0.0	0.0	0.0	0.0	0.0	100.0	0.272	0	3	100	7.3
65-69 years	90.3	5.5	2.9	1.3	0.0	0.0	0.0	0.0	0.0	100.0	0.153	0	3	64	4.6
More than 69 years	97.7	0.0	2.3	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.047	0	2	42	3.0
All	48.0%	17.0%	13.2%	8.7%	5.5%	3.6%	2.4%	0.9%	0.8%	100.0%	1.369	0	11	2012	100.0%

TauB= -0.4120
Gamma= -0.5435

Table A6.22

FREQUENCY OF ANY RESIDENTIAL MOVE BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Residential Moves											Range			
Age in 1968	0	1	2	3	4	5	6	7	8 or More	Total	Mean	Minimum	Maximum	Number of Observations	Weighted Percentage of Observations
Less than 30 years	12.4%	24.9%	18.6%	14.9%	9.7%	8.2%	5.3%	3.2%	2.8%	100.0%	2.639	0	9	420	19.4%
30-39 years	42.1	23.8	11.5	11.6	5.3	2.2	1.8	1.5	0.2	100.0	1.370	0	8	531	24.1
40-49 years	59.2	22.3	7.0	5.2	3.9	1.7	0.3	0.3	0.1	100.0	0.808	0	8	520	25.2
50-59 years	62.0	20.3	11.0	3.1	1.3	0.9	1.4	0.0	0.1	100.0	0.701	0	9	335	16.4
60-64 years	56.1	34.7	6.3	1.5	0.5	1.0	0.0	0.0	0.0	100.0	0.584	0	5	100	7.3
65-69 years	65.2	16.3	9.8	3.0	4.2	0.0	1.4	0.0	0.0	100.0	0.704	0	6	64	4.6
More than 69 years	65.1	32.6	2.3	0.0	0.0	0.0	0.0	0.0	0.0	100.0	0.373	0	2	42	3.0
All	46.7%	23.7%	10.9%	7.7%	4.6%	2.8%	1.8%	1.0%	0.6%	100.0%	1.248	0	9	2012	100.0%

TauB= -0.3226
Gamma= -0.4243

Table A6.23

FREQUENCY OF THE TOTAL NUMBER OF EVENTS BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Total Number of Events												Range		Number of Observa-tions	Weighted Percentage of Observa-tions
	0	1-3	4-6	7-9	10-12	13-15	16-20	21-25	26-35	36-48	Total	Mean	Mini-mum	Maxi-mum		
Less than 30 years	0.0%	2.8%	12.5%	15.5%	16.4%	13.8%	16.1%	11.2%	10.0%	1.3%	100.0%	14.550	1	48	420	19.4%
30-39 years	1.0	11.3	21.8	16.3	14.4	13.5	11.4	4.8	4.9	0.3	100.0	10.796	0	43	531	24.1
40-49 years	1.4	11.3	24.5	20.6	15.7	8.5	10.7	4.8	2.6	0.0	100.0	9.721	0	33	520	25.2
50-59 years	0.4	9.7	29.8	28.1	13.8	9.3	4.9	3.3	0.6	0.1	100.0	8.528	0	38	335	16.4
60-64 years	1.0	18.3	34.7	21.1	10.5	8.5	5.6	0.1	0.0	0.0	100.0	7.179	0	23	100	7.3
65-69 years	4.6	39.3	22.3	18.8	6.6	5.5	2.7	0.0	0.0	0.0	100.0	5.274	0	19	64	4.6
More than 69 years	0.0	40.3	39.7	11.4	6.3	2.3	0.0	0.0	0.0	0.0	100.0	4.706	1	13	42	3.0
All	0.9%	12.1%	23.5%	19.5%	14.2%	10.6%	10.0%	5.0%	3.9%	0.3%	100.0%	10.184	0	48	2012	100.0%

TauB= -0.2718
Gamma= -0.3106

Table A6.24

FREQUENCY OF THE TOTAL NUMBER OF DIFFERENT EVENTS BY AGE, 1968-1978

(For all married male household heads in 1968.)

Age in 1968	Number of Different Events									Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3	4-5	6-8	9-12	13-19	Total		Minimum	Maximum		
Less than 30 years	0.0%	1.2%	1.5%	6.2%	19.6%	34.0%	29.8%	7.7%	100.0%	7.552	1	19	420	19.4%
30-39 years	1.3	3.1	7.0	10.9	25.6	31.5	15.2	5.5	100.0	6.160	0	17	531	24.1
40-49 years	1.3	4.7	7.1	16.1	28.3	25.1	15.0	2.4	100.0	5.480	0	17	520	25.2
50-59 years	0.9	2.7	5.2	10.5	34.6	32.2	11.8	2.3	100.0	5.712	0	16	335	16.4
60-64 years	1.0	2.0	10.6	16.2	35.5	23.1	11.5	0.0	100.0	5.026	0	11	100	7.3
65-69 years	4.6	21.5	13.4	13.6	23.0	16.6	7.1	0.0	100.0	3.710	0	11	64	4.6
More than 69 years	0.0	13.9	17.1	18.7	32.2	15.8	2.3	0.0	100.0	3.742	1	11	42	3.0
All	1.1%	4.2%	6.5%	12.0%	27.4%	28.7%	16.3%	3.7%	100.0%	5.919	0	19	2012	100.0%

TauB= -0.2156
Gamma= -0.2521

Table A6.25

FREQUENCY OF THE THE TOTAL NUMBER OF DESIRABLE EVENTS BY AGE, 1968-1978
(For all married male household heads in 1968.)

Age in 1968	Number of Desirable Events 0	1	2	3	4-5	6-10	11-15	Total	Mean	Range Mini-mum	Range Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	4.0%	14.6%	18.5%	13.8%	20.0%	25.1%	4.0%	100.0%	4.132	0	15	420	19.4%
30-39 years	24.9	22.7	16.5	12.6	12.5	10.6	0.2	100.0	2.242	0	12	531	24.1
40-49 years	38.7	28.3	13.2	8.8	6.2	4.8	0.0	100.0	1.406	0	9	520	25.2
50-59 years	46.1	24.1	13.4	9.7	4.9	1.7	0.1	100.0	1.122	0	11	335	16.4
60-64 years	40.5	31.9	17.2	8.0	1.4	1.0	0.0	100.0	1.018	0	6	100	7.3
65-69 years	53.2	21.0	14.5	4.5	5.3	1.4	0.0	100.0	0.979	0	7	64	4.6
More than 69 years	46.1	42.7	8.9	0.0	2.3	0.0	0.0	100.0	0.698	0	4	42	3.0
All	30.9%	23.9%	15.3%	10.3%	9.7%	9.4%	0.8%	100.0%	2.022	0	15	2012	100.0%

TauB= -0.3506
Gamma= -0.4329

Table A6.26

FREQUENCY OF THE NUMBER OF DIFFERENT DESIRABLE EVENTS BY AGE, 1968-1978

(For all married male household heads in 1968.)

Age in 1968	Number of Different Desirable Events						Mean	Range		Number of Observations	Weighted Percentage of Observations
	0	1	2	3	4	Total		Minimum	Maximum		
Less than 30 years	4.0%	24.9%	45.2%	19.9%	6.0%	100.0%	1.990	0	4	420	19.4%
30-39 years	24.9	35.3	27.5	10.4	1.9	100.0	1.290	0	4	531	24.1
40-49 years	38.7	38.0	16.5	6.3	0.5	100.0	0.919	0	4	520	25.2
50-59 years	46.1	32.5	17.4	3.7	0.3	100.0	0.797	0	4	335	16.4
60-64 years	40.5	40.5	19.0	0.0	0.0	100.0	0.785	0	2	100	7.3
65-69 years	53.2	35.6	8.5	2.7	0.0	100.0	0.607	0	3	64	4.6
More than 69 years	46.1	45.1	6.5	2.3	0.0	100.0	0.651	0	3	42	3.0
All	30.9%	34.2%	24.4%	8.8%	1.8%	100.0%	1.165	0	4	2012	100.0%

TauB= -0.3411
Gamma= -0.4429

Table A6.27

FREQUENCY OF THE TOTAL NUMBER OF UNDESIRABLE EVENTS BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Undesirable Events									Range		Number of Observations	Weighted Percentage of Observations
Age in 1968	0	1	2	3	4-5	6-10	11-19	Total	Mean	Minimum	Maximum		
Less than 30 years	26.5%	19.1%	14.9%	10.0%	14.6%	11.8%	3.3%	100.0%	2.727	0	18	420	19.4%
30-39 years	32.1	18.1	13.2	9.5	13.0	11.6	2.4	100.0	2.476	0	19	531	24.1
40-49 years	28.9	18.9	13.8	11.0	11.6	11.4	4.3	100.0	2.723	0	16	520	25.2
50-59 years	23.2	24.1	17.9	12.5	12.5	7.7	2.2	100.0	2.331	0	17	335	16.4
60-64 years	26.4	34.3	17.9	9.3	9.1	2.9	0.1	100.0	1.639	0	11	100	7.3
65-69 years	22.5	22.9	25.1	22.4	7.2	0.0	0.0	100.0	1.701	0	5	64	4.6
More than 69 years	4.3	23.4	39.3	21.6	11.5	0.0	0.0	100.0	2.148	0	5	42	3.0
All	27.1%	21.0%	16.1%	11.4%	12.3%	9.4%	2.6%	100.0%	2.457	0	19	2012	100.0%

TauB= -0.0246
Gamma= -0.0300

Table A6.28

FREQUENCY OF THE NUMBER OF DIFFERENT UNDESIRABLE EVENTS BY AGE, 1968-1978
(For all married male household heads in 1968.)

	Number of Different Undesirable Events									Range			
Age in 1968	0	1	2	3	4	5	6-7	Total	Mean	Mini-mum	Maxi-mum	Number of Observa-tions	Weighted Percentage of Observa-tions
Less than 30 years	26.5%	27.0%	18.6%	16.4%	6.7%	3.3%	1.6%	100.0%	1.662	0	7	420	19.4%
30-39 years	32.1	22.7	22.5	13.4	6.8	1.3	1.4	100.0	1.496	0	7	531	24.1
40-49 years	28.9	25.0	26.1	10.1	5.8	3.7	0.3	100.0	1.512	0	7	520	25.2
50-59 years	23.2	31.7	22.6	13.6	5.8	1.7	1.4	100.0	1.577	0	6	335	16.4
60-64 years	26.4	41.3	16.6	12.3	3.3	0.0	0.0	100.0	1.248	0	4	100	7.3
5-69 years	22.5	37.2	26.5	10.9	2.9	0.0	0.0	100.0	1.345	0	4	64	4.6
More than 69 years	4.3	44.4	32.4	18.8	0.0	0.0	0.0	100.0	1.658	0	3	42	3.0
All	27.1%	28.3%	22.7%	13.2%	5.7%	2.1%	0.9%	100.0%	1.525	0	7	2012	100.0%

TauB= -0.0146
Gamma= -0.0185

Chapter 7

SEX DIFFERENCES IN MEASUREMENT ERROR IN STATUS ATTAINMENT[1]

Mary Corcoran

INTRODUCTION

Empirical analyses of status attainment typically measure background by asking respondents to recollect their parents' or family's characteristics (e.g., father's usual occupation while growing up). A major criticism of such analyses is that retrospective reports of a parent's status may be less reliable than are contemporaneous reports of the respondent's own status and that, as a result, parameter estimates may be biased (Bowles, 1972; Bowles and Nelson, 1974).

A number of researchers have attempted to estimate response error models for such reports of parental status and to incorporate them into their structural equation models of the male status attainment process (Bielby, Hauser and Featherman, 1977a; Bielby, Hauser, and Featherman, 1977b; Corcoran, 1979; Mason et al, 1976). Several studies have estimated separate models of the relationships between offsprings' reports and the true values of parental traits for different race or age-race groups for men. None examined measurement error patterns for women. Only one (Mare and Mason, 1978) examined the pattern and strength of such relationships across populations as well as within populations. As Mare and Mason pointed out, estimating models across groups has the advantage that one can test explicitly for intergroup differences by comparing a model which constrains group parameters (or parts of them) to be the same, to a model which allows parameters (or parts of them) to vary across groups. Further, by exploiting intergroup similarity in measurement models, one can obtain more reliable parameter estimates.[2]

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[1] This paper has benefitted greatly from comments by Dwayne Alwin, Robert Ferber, Robert Hauser, Kent Jennings, Otis Dudley Duncan and Robert Mare. The analysis strategy follows that developed by Mare and Mason (forthcoming) in their investigation of age differences in quality of boys' reports of parental status. Many of the technical discussions are informed by those in Mare and Mason (forthcoming) and by those in Bielby, Hauser, and Featherman (1977). None of the above individuals are responsible for any opinions or errors in this paper.

A number of researchers have proposed and estimated models of women's status attainment and/or of differences in status attainment between men and women (Alexander and Eckund, 1974; Dejong, 1971; Featherman and Hauser 1974, 1976; Shaw and Sewell, 1967; Suter and Miller 1973; Trieman and Terrell, 1975; Tyre and Treas 1974). But few (if any) have explicitly considered how their results might be distorted by measurement error in women's retrospective reports of parental status and/or by sex differences in such measurement error.

At first glance, it seems unlikely that the quality of retrospective reports of parental traits would vary by sex. Yet, there are at least three plausible reasons to expect some variation by sex: sex differences in verbal contacts with parents; sex differences in expectations about how status is attained and sex-role identification with the same-sex parent.

Goldberg and Lewis (1969) reported that by as early as 13 months of age girls talked to their mothers more often than boys did. Unpublished data from Kent Jennings' longitudinal study of adolescent political socialization suggest that these sex differences in verbal contact still exist in adolescence. These sex differences in verbal interactions with parents might improve girls' later recall relative to that of boys.

Some have argued that men's and women's mobility occurs through different channels--men attain status through individual achievement, while women derive status from kinship patterns (Blake, 1974). This assumption is implicit in the relative dearth of work until quite recently on women's status attainment. Even this recent work considers mobility through marriage as well as through individual achievement. If boys and girls have different expectations about status attainment, with girls expecting to derive status from kinship patterns to men, then parental status may be more salient for young women.

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[2] Take for instance, Mare and Mason's analysis of measurement errors in children's reports of parental traits. They compared parents' and children's reports for sixth, ninth and twelfth graders. They assumed that the relationship between parental reports and true values of parental traits did not vary by children's grade level, but allowed relationships between children's reports and true values to vary. Pooling groups increased the number of observations, while the between-grade constraints on relationships between parental reports and true values reduced the degrees of freedom used.

Children may take the same sex parent as a role model. Thus, a father's characteristics may be more salient for boys and a mother's characteristics may be more salient for girls.

If the quality of retrospective reports of parental status varies by sex, this could systematically bias parameter estimates in structural equation models of sex differentials in the early status attainment process. This could lead either to exaggerations or underestimates of the importance of sex differences in the status attainment process--depending on how the measurement error operates.

This chapter explores sex differences in retrospective reports of parental traits using a multiple group measurement model of young adults' reports of parental status similar to that employed by Mare and Mason. The discussion which follows will:

1) describe the extent and patterns of measurement error in women's and men's reports of their parents' status traits;
2) test whether the structures of men's and women's measurement errors differ;
3) test whether women's and men's retrospective reports of parental traits are less reliable than are their parents' contemporaneous reports of these traits;
4) explore the extent to which corrections for measurement error alter estimates of male/female differences in the educational attainment process.

Sample

The Panel Study of Income Dynamics (PSID) is used to study sex differences for measurement error in retrospective reports of parental status. The Panel Study is well suited for such an exploration since it follows families over time and takes separate interviews with children who leave home.

The sample for this analysis included all non-institutionalized white male household heads, female household heads and wives aged 23-30 in 1976 who had been living with both parents in 1968. The original sample included 316 men and 280 women. I chose this age range because the majority of children leave home by age 23.[3] Thus, this is a nationally representative sample of children aged 16 to 22 in 1968 and living with both parents who left home sometime over the next eight years. Since these young adults had all been living with their parents prior to the eight-year period, they would be expected to report parental characteristics more accurately than do older adults, and this analysis should provide a lower bound on the extent of retrospective error which would occur for individuals in a broader age range.

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[3] Hill (1977) reports that the majority of young men and young women aged 23 were not living at home in 1970.

Respondents were dropped from this sample if:

1) their fathers did not report their own education, their occupation, or their wives' education in 1968;
2) their mothers were not interviewed in 1976;
3) their mothers did not report their education in 1976;
4) the respondents did not report their own or their father's education, father's occupation or mother's education.

This final sample provided complete information for the following variables:

1) father completed education in years in 1968 as reported by father in 1968, (ED_{f-f});
2) father's occupation in 1968, as reported by father in 1968, (OCC_{f-f});
3) mother's completed education in 1968 as reported by father in 1968, (ED_{m-f});
4) mother's reports of her education in 1976, (ED_{m-m});
5) respondent's report of his or her father's education (ED_{f-o}); of the father's usual occupation while respondent was growing up, (OCC_{f-o}); and of mother's education (ED_{m-o}).

Table 7.1 describes how the restrictions reduced the sample size and affected means and variances. The original sample included 280 white women and 316 white men. Of these, 222 (79.3 percent) of the women and 264 (83.5 percent) of the men had fathers who reported their own education and occupation and their wives' education and had mothers who reported their own schooling in a 1976 interview. Finally, 208 (74.2 percent) of the women and 217 (68.7 percent) of the men reported on all three parental characteristics.

Note the difference in the extent of missing data by sex. Very few women, only about 6 percent (14/222 = .06), failed to report on all three parental traits. Men were much more likely not to report, about 18 percent (47/264 = .18) failed to report on all three parental traits. If people with missing data on one question were more likely than other people to misreport on another question, then dropping missing data should raise the reliability of sons' reports relative to those of daughters'.

Table 7.1

EFFECTS OF SAMPLE RESTRICTIONS ON MEANS AND STANDARD DEVIATIONS[a]
(For all non-institutionalized white male
household heads, female household heads and wives who lived with
both parents in 1967)
(standard deviations in parentheses)

Sample Limitations[b]	N	Weighted Percent	Father's Occupation as Reported by Father in 1968[c]	Father's Education as Reported by Father in 1968	Mother's Education as Reported by Father in 1968
Men					
-	316	100.0	37.1 (19.8)	10.75 (3.50)	10.94 (2.69)
1	264	83.5	36.7 (20.5)	10.78 (3.61)	10.97 (2.87)
1,2	217	68.7	37.3 (20.5)	10.94 (3.47)	11.06 (2.77)
Women					
-	280	100.0	37.6 (20.5)	10.88 (3.29)	11.37 (2.85)
1	222	79.3	36.6 (20.3)	11.35 (2.79)	11.31 (2.76)
1,2	208	74.3	36.9 (20.6)	11.01 (3.27)	11.51 (2.77)

[a] Whenever a variable has missing data, that case is omitted from calculations.

[b] 1 = Father reported his education, occupation and wife's education in 1967 and wife in 1967 was interviewed in 1976 and reported her education. (Mothers who left the sample or died between 1968 and 1976 are dropped.)

2 = Splitoff reported father's education, father's occupation and mother's education.

[c] Occupations are coded into the eight broad Census categories and then transformed into Duncan scores.

The overall effect of sample restrictions on means and variances of parental traits (as reported by the father) was quite small. For sons, variances increased slightly; for daughters, variances remained much the same. For both men and women, the means on both parental education measures increased slightly, while the mean value of father's occupation dropped for women and increased slightly for men.

Timing, Wording and Coding of Parental Status Questions

This analysis estimates the reliability of reports of parental status traits by four different reporters: father, mother, son, daughter. The timing and wording of questions varied somewhat by the type of reporter.[4] In 1968 fathers reported on their education, their current occupation, and their wives' schooling. Mothers reported on their own schooling in 1976. In the year they left home, sons reported on their parents' status traits.[5] Given the timing of interviews, this means that they could have left home (or school)[6] anywhere from one day to 18 months previously. Daughters, if married in 1976, reported in 1976 on their parent's traits. Daughters, who were heading their own households in 1976 reported on their parents' traits in the year in which they first became a household head. This means that daughters reported on their parents' status anywhere from one day to seven and one-half years after they left home (or school). Thus, daughters, on average, had been out of their parental home longer than had sons when they reported on parental traits. If recall diminishes over time, these timing differences might lower the reliabilities of daughters' reports relative to those of sons', and might lower reliabilities of offsprings' reports relative to those of their parents.

The questions asked about parental traits, and the codes used are listed in Appendix A.2. Detailed, multiple item protocols were used when fathers and mothers reported their own schooling and when fathers reported their wives' schooling. Offspring, on the other hand, were asked only two questions about their parents' schooling. The schooling questions were worded differently for parents and children. Parents were asked "How many grades of school did you finish?" while offspring were asked, "How much education did your father (mother) have?" Coding procedures were quite similar for offspring and parents.[7]

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[4] I am grateful to Professor Robert Hauser for pointing out in a personal communication (June, 1979) that these differences might affect reliabilities. The above discussion borrows heavily from points raised by Hauser.

[5] There is one exception. Sons reported on mother's schooling in 1974 if they left home prior to 1974.

[6] They may also have just left the armed forces, jail or an institution.

It is unclear whether such differences in the number and wording of schooling questions would affect data quality, but I suspect that any such effects should operate to the parents' advantage. Estimates of sex differences in measurement error structure should be unaffected since the number and wording of questions about parental traits were the same for men and women.

Fathers and offspring were asked different questions about father's occupation. The father's question sequence was more detailed (a question and a probe) than the offspring's single question, and the wording of questions differed. Fathers were asked to report their "main occupation in 1968" (when offspring were 15 to 22 years old); offspring were asked, "What was your father's usual occupation while you were growing up." Coding rules were the same for fathers and offspring. Recall that I use the Duncan score of father's occupation as a measure of the average occupational status of the father in the period during which children were growing up. Although the fathers' and children's questions did not attempt to ascertain occupation for precisely the same time period, all the measures gave us a fix on father's average occupational status. Indeed, one might argue that the offspring's question was more directly aimed at the desired characteristic. If so, this should raise the reliability of offsprings' reports relative to that of fathers' reports. On the other hand, the probe in the father's question should tend to increase the reliability of the father's report relative to that of his children.

Sample Advantages and Disadvantages

These samples have both advantages and disadvantages relative to other populations used in the analysis of measurement error. On the plus side, each has an extremely rich set of indicators of parental status. Included were parents' reports of their own characteristics and children's reports of these characteristics. These reports were obtained at different times (at least one interview year apart), the children were no longer living with their parents when they reported on parental traits, and the fathers described their own characteristics during a period when the children were still living at home. Second, the sample sizes were large enough to estimate a fairly detailed model with some precision. Third, there is complete data on all the variables of interest. On the other hand, the sample is restricted to young adults who were 16 to 22 years old in 1968, who were living at home in 1968 and who left home between 1968 and 1976. That is, these young adults were living in intact families in 1968 and reported on their parents' traits within eight years (one day to 7 1/2 years) after leaving home. The quality of their retrospective reports would be expected to be at least as high as that of adults in a wider age range or in a sample which included children from broken homes. That is, this sample should provide a good lower bound estimate of a reliability for retrospective reports of parental status. There is also a problem with missing data. About 31 percent of the sample of parent-son pairs and 26 percent of the sample of parent-daughter pairs were dropped because of missing data so that results may not be easily generalized. But most studies of measurement error have problems with missing data, and alternatives to excluding cases with missing data (e.g., use of pairwise correlations, assigning values) are equally limiting since they often involve dubious assumptions and are likely to misrepresent the true error structure.[8]

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[7] Again there is one major exception. Specifically, offspring who reported that they did not know their parents' schooling were asked if their parents could read or write. Those who responded "yes" were coded as having six to eight years of schooling. Parents who reported they "didn't know" their (or a spouse's) schooling were not asked a literacy question. I dealt with this problem by looking up all cases where a son or daughter was coded as having a value of 6-8 years on a parental schooling measure. There were five cases for men and five for women where the original response had been "don't know." These were then recoded as "DK, NA," and so dropped from analysis because of missing data.

Finally, the timing, number, and wording of questions about parental traits differed by type of respondent. Parents reported on their own (or on a spouse's) current traits while children reported retrospectively on a parent's traits. In addition, parents were asked more questions and more detailed questions. This may have decreased the observed reliability of offsprings' reports relative to those of their parents. To the extent that such differences affect relative reliabilities, they would be likely do so by increasing the reliability of parents' reports relative to those of their offspring. Thus, if the estimated reliabilities of offsprings' and parents' reports are about equal, one may reasonably conclude that differences in the timing and the number and the wording of questions had trivial effects on reliabilities. On the other hand, if offsprings' reports were consistently less reliable than those of the parents, one could not distinguish between competing explanations. Daughters and sons were asked the same questions, but the timing of questions differed. Sons answered these questions within one day to 18 months after leaving home (or school), while daughters' response periods varied from one day to seven and one-half years after leaving home (or school). This may have lowered the accuracy of daughters' reports relative to those of sons.

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[8] I decided to eliminate all cases with missing data on one or more variables of interest. There were other options. I could have used pairwise correlations or I could have assigned values to missing data. Any procedure would involve problems. Bielby, Hauser and Featherman (1978) described the following problems: Use of pairwise correlations assumes that these accurately reflect the correlations that would be present in the absence of missing data (Bielby et al used this procedure). Assigning means for missing data values reduces variances and error variances. Random assignment of missing data reduces ability to find random errors; systematic assignment would have the opposite effect. Omitting missing data (the technique used in this paper) reduce the sample size by 30 percent and perhaps selected out many of the less accurate responses. If this is true, these analyses would underestimate the error in reports of background traits. See Griliches, Hall, and Hausman (1978) for an extensive discussion of these issues.

Statistical Models

A Single Group Measurement Model

For each sex, the basic measurement model can be described by the following seven equations:[9]

$$(1)\quad ED_{f-f} = \lambda_{1f}\, ED_{f-t} + \varepsilon_{1f}$$

$$(2)\quad ED_{f-o} = \lambda_{1o} ED_{f-t} + \varepsilon_{1o}$$

$$(3)\quad OCC_{f-f} = \lambda_{2f} OCC_{f-t} + \varepsilon_{2f}$$

$$(4)\quad OCC_{f-o} = \lambda_{2o} OCC_{f-t} + \varepsilon_{2o}$$

$$(5)\quad ED_{m-f} = \lambda_{3f} ED_{m-t} + \varepsilon_{3f}$$

$$(6)\quad ED_{m-o} = \lambda_{3o} ED_{m-t} + \varepsilon_{3o}$$

$$(7)\quad ED_{m-m} = \lambda_{3m} ED_{m-t} + \varepsilon_{3m}$$

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[9] This model is described in more detail in Corcoran (1979).

where

ED i-j = the j'th person's report of the i'th person's education

OCC i-j = the Duncan score of the j'th person's report of the i'th person's occupation

i,j = f for father
o for offspring
m for mother
t for true

These algebraic equations specify that the jth person's report of the ith parental status characteristic is generated by the true score of that characteristic times λ_{ij} plus a response errors (ε_{ij}) that is independent of the true score. To identify this model I set $\lambda_{1f} = \lambda_{2f} = \lambda_{3m} = 1$. The values of the other four λ's (λ_{1o}, λ_{2o}, λ_{3f}, λ_{3o}) were estimated. Thus, I could not determine the absolute values of the λ_{kj}, but only their relative values.

For each true parental status trait, this is a congeneric model which allows for correlated errors (Joreskog, 1969; Alwin and Jackson, forthcoming). In the full model, true scores were allowed to covary. Response errors were allowed to covary or were set at zero depending upon our hypotheses about the extent to which individuals use information about one trait in reporting another. Parameter estimates were used to calculate reliabilities (Joreskog). For example, the reliability of the offspring's report of the father's schooling (ED_{f-o}) can be estimated as

$$R = \frac{\lambda_{1o}^2 \text{ Var } (ED_{f-t})}{\lambda_{1o}^2 \text{ Var } (ED_{f-t}) + \text{Var } (\varepsilon_{1o})}$$

This model has two important limitations. (See Mare and Mason, for a more complete discussion.) First, if I estimated models separately, we could not make an explicit statistical test for sex differences in measurement error. Second, failing to explore intergroup similarities would result in inefficient parameter estimates. Specifically, estimating models separately by sex would produce two sets of parameter estimates for the covariances among true parental characteristics and for the error variances of parental reports, thus estimates of parental reliabilities would vary by sex of child. But there is no reason to expect that the accuracy of parents' reports would vary with the sex of their child. Combining information for both sexes to estimate a single set of true parental traits covariances and parental report reliabilities should increase the number of observations and degrees of freedom and hence the precision of our estimates.[10]

A Multiple Group Measurement Model

The shortcomings of the single group model approach can be handled by using Joreskog's (1971) general framework for the simultaneous covariance structure analysis of multiple populations. This framework allows one to constrain some parameters to vary by sex of child and others to be invariant. For instance, one can specify that the reliabilities of parents' reports and that the joint distribution of true parental characteristics are the same for sons and daughters, while allowing the reliabilities of offsprings' reports to vary by sex of offspring.

Applying Joreskog's model to this problem was straightforward. I estimated equations 1 through 7 for sons and for daughters. This gave a model of the form:

$$(8)\quad ED_{f-f-k} = ED_{f-t-k} + \varepsilon_{1fk}$$

$$(9)\quad ED_{f-o-k} = \lambda_{fok}\, ED_{f-t-k} + \varepsilon_{2ok}$$

$$(10)\quad OCC_{f-f-k} = OCC_{f-t-k} + \varepsilon_{2fk}$$

$$(11)\quad OCC_{f-o-k} = \lambda_{2ok}\, OCC_{f-t-k} + \varepsilon_{2ok}$$

$$(12)\quad ED_{m-f-k} = \lambda_{3fk}\, ED_{m-t-k} + \varepsilon_{3fk}$$

$$(13)\quad ED_{m-o-k} = \lambda_{3ok}\, ED_{m-t-k} + \varepsilon_{3ok}$$

$$(14)\quad ED_{m-m-k} = ED_{m-t-k} + \varepsilon_{3mk}$$

where k = d for daughters and k = s for sons.

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[10] Note this was the strategy followed by Mare and Mason in their analysis of response errors in boys' reports of parental status by grade level.

The remainder of this section briefly outlines the various forms of this general model which were estimated. Appendix A.2 describes both the general model and the various forms in detail for those interested.

Specification of Errors in Parents' Reports across Sons and Daughters

In order to specify that the joint distribution of true parental status and that the reliabilities of parents' reports did not vary by sex of offspring, I equated the true score covariance matrices (of parental status traits) for each sex group; I equated the slope of father's reports of mother's schooling on its true values for each sex group; and I equated the error variances of the equations for parents' reports of the three parental status characteristics across sons and daughters.[11] In addition, when errors in parent's reports are allowed to covary then these covariances also were equated across sons and daughters.

Specification of Error Covariances

I first investigated three alternative error patterns.[12]

1) All measurement errors were random; that is, errors in reports of parental traits were uncorrelated.
2) Offspring could guess at one parental status characteristic based on their knowledge of other parental status traits (Mason et al., pages 439-461).[13] Offspring could overstate the consistency in parental

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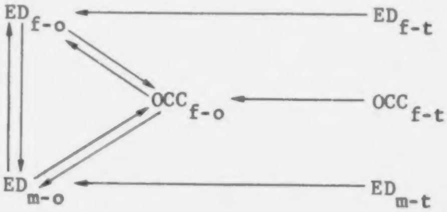
[11] This strategy is similar to that employed by Mare and Mason.

[12] These first two specifications are equivalent to models tested by Bielby, et al., and by Mare and Mason on different populations.

[13] If offspring guessed at one parental trait based on knowledge of other parental traits, this would imply a simultaneous model of the form.



Such a model could not be identified with these data. But, if reciprocal causation were occurring, the response errors should be correlated (Mason et al.)

status traits (Bielby et al.'s within-occasion between-variable correlated error). This should result in correlations between the children's reporting errors (ε_{1ok}, ε_{2ok}, ε_{3ok}). Such error correlations may be an artifact of data collection procedures.[14] For instance, offsprings' reports of mothers' and fathers' schooling were elicited in response to similar sets of questions. Other studies show some evidence of correlated errors in offsprings' reports; Mare and Mason (1978) and Corcoran (1979) reported significant correlations between errors in sons' reports of mothers' and fathers' schooling.

3) Fathers could overstate the consistency between their own and their wives' status traits. This should result in correlations between fathers' reporting errors of their own and of their wives' schooling (ε_{1fk}, ε_{3fk}).

Other possible error patterns were less likely. Men might have overstated the consistency in their own status characteristics so that the errors in fathers' reports of their schooling and occupation might covary. Bielby et al. investigated this type of non-random error in some detail and found no evidence of such error in their analysis. Offsprings' and parents' reports of parental traits might covary; but since reports were taken several years apart, it is difficult to see how this could have occurred.

To summarize, three models of measurement error were posited. Model I allowed only random measurement errors; the ε_{ijk} were assumed to be uncorrelated with each other. Model II allowed for correlations between errors in offspring's reports of different parental traits. This model tested whether sons overstated the consistency between parental status traits. The following correlations are estimated:

$$r_{\varepsilon_{1ok},\ \varepsilon_{2ok},\ 3ok}\ ;\ r_{\varepsilon_{2ok},\ 3ok}\ .$$

Model III allowed errors in fathers' reports of their own and of their wives' schooling to covary. This tested whether men tended to make reports of their own and of their wives' schooling consistent. One correlation was estimated:

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[14] Both Mare and Mason and Hauser (in a personal communication) suggest this possibility.

$r_{\varepsilon_{1fk,\ 3fk}}$.

Models I and III are each overidentified. Models II and III can be merged into a new model (Model IV) without the new model becoming underidentified, so that these models can be nested. Two models are nested if one can be obtained from the other by fixing some parameters in the other model. Joreskog's maximum likelihood estimation procedure only allows evaluation of competing models when these models are nested.

Test for Sex Differences in Measurement Error

In models I to IV, I did not impose any sex equality constraints on offsprings' measurement error structures. Joreskog's multiple group model permits a direct test of such differences. After choosing the best-fitting model from Models I to IV, I tested for sex differences in measurement error structures by comparing that model to a model which freed the same error covariances as the best-fitting model, but which constrained male and female offsprings' measurement error structures to be equal. If the constrained model did not provide a significantly worse fit to the data, then one could conclude that any observed sex differences in the quality of retrospective reports could have arisen from sampling variability.

Procedure for Selecting a Model

In addition to using goodness-of-fit statistics, I selected models by substantive plausibility.[15] In particular, I specified that children's retrospective reports of parental status should be no more reliable than parents' self-reports. If parameter estimates implied that this were not so, then it would be likely to be due either to a misspecified model or to sampling variability, and I would reestimate the models using a specification which equates parents' and offsprings' reliabilities. Similarly, it seemed reasonable to specify that mothers' reports of their own schooling should be at least as reliable as are fathers' reports of their wives' schooling.

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[15] Again this is similar to the strategy followed by Mare and Mason, 1978.

Procedures for Examining Parent/Child and Male/Female Differences in Reliabilities

Once I had chosen the best fitting plausible model, I investigated differences in estimated reliabilities between offspring and parents and between men and women. I did this by comparing models which equate slopes (λ's) and error variances for particular groups (i.e., parallel measures models) to models which allowed these reliabilities to vary across groups.[16] Whenever constraining reliabilities to be equal did not provide a significantly worse fit to the data, I could not reject the hypothesis that the observed group differences in reliabilities could have arisen from sampling variability.

Results

Choice of a Best Fitting Model

Table 7.2 reports the likelihood ratio-tests for Models I, II, III, and IV which were estimated using Joreskog's general procedure for the simultaneous covariance structure analysis of multiple populations. The likelihood-ratio tests associated with two nested models could be used to establish which of these two would best reproduce the observed data.[17] This difference is distributed as a chi-square distribution with degrees of freedom equal to the number of parameters free in one model and fixed in the other. When the value of chi-square was significant, this meant that the model with the most free parameters provided a significantly better "fit" to the data.

A comparison of Models I and II showed that allowing offsprings' reporting errors of different parental traits to covary resulted in a chi-square of 14.94 (38.43-23.49) with 6 (33-27) degrees of freedom. This improvement in fit was statistically significant and suggested that offspring tended to make reports of parental status consistent with one another.

Further allowing errors in fathers' reports of their own and their wives' schooling to covary (Model IV) resulted in a significant chi-square of 7.04 (23.49-16.45) with 1 degree of freedom. Apparently, men tended to make reports of their own and their wives' schooling consistent with one another.

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[16] Reliabilities were constrained to be equal for two reporters by equating slopes (λ_{ik}) and error variances (ε_{ik}) for those reporters. This was the strategy followed by Mare and Mason.

[17] See Bielby et. al, 1977a, 1977b.

TABLE 7.2 (Page 1 of 2)
GOODNESS OF FIT STATISTICS FOR MEASUREMENT MODELS
(For non-institutionalized, white household heads and wives, aged 23-30 in 1975 who lived with both parents in 1967 and who reported their parents' status characteristics)

Model		X^2	Degrees of Freedom	Probability Level
I.	Random measurement errors in offspring's reports of parental status	38.43	33	.237
II.	Covariance among all offsprings' errors for men and women	23.49	27	.658
III.	Covariance among errors in fathers' reports of their own and their wives' schooling	31.06	32	.514
IV.	Covariances among all offsprings errors for men and women and among errors in fathers' reports of their own and their wives' schooling	16.45	26	.925
V.	Model IV with sons' and daughters' measurement error structures constrained to be equal	35.64	35	.438

Table 7.2 (page 2 of 2)

Model		X^2	Degrees of Freedom	Probability Level
VI.	Covariances between errors in reports of fathers' education and mothers' education for both male offspring and fathers	20.35	31	.928
VII.	Model VI with the reliabilities of fathers' and daughters' reports of fathers' schooling constrained to be equal, with the reliabilities of fathers' and daughters' reports of fathers' occupation constrained to be equal and with the reliabilities of fathers' and mothers' reports of mothers' schooling constrained to be equal	26.80	37	.892
VIII.	Model VII with reliabilities of parents' and daughters' reports of mothers' education constrained to be equal	28.68	39	.888
IX.	Model VIII with reliabilities of sons' reports of fathers' occupation and of fathers' education constrained to equal reliabilities of fathers' and of daughters' reports	36.62	43	.743
X.	Model IX with reliabilities of sons' reports of mothers' education constrained to equal reliabilities of daughters' and parents' reports	60.27	45	.064

Model V constrained Model IV so that sons and daughters had identical measurement error structures. All error variances were constrained so that error covariances and slopes were equal across sexes. Model V provided a significantly worse fit ($\chi^2 = 19.29$ with 9 degrees of freedom) to the data than did Model IV, suggesting that measurement error structures differed by sex.

Model IV provided the best fit among Models I to V. In Model IV, only two estimated covariances significantly differed from zero (using the .05 level): the covariances between errors in sons' reports of fathers' and of mothers' schooling and the covariances between errors in fathers' reports of fathers' and of mothers' schooling. I simplified Model IV by setting all insignificant error covariances to zero. This new model (Model VI) did not provide a significantly worse fit to the data than did Model IV.

Under Model VI, the estimated reliabilities of daughters' reports of fathers' education (.837) and of fathers' occupation (.842) exceeded the estimated reliabilities of the fathers' own reports (.815 and .832, respectively) and the estimated reliabilities of fathers' and mothers' reports of mothers' schooling were approximately equal (.902 vs. .892). I simplified Model VI by equating the reliabilities of daughters' and fathers' reports of fathers' schooling, of daughters' and fathers' reports of father's occupation, and of fathers' and mothers' reports of mothers' schooling (Model VII). Model VII did not provide a significantly worse fit to the data than did Model VI ($\chi^2 = 5.55$ with 6 degrees of freedom). Thus, the apparent higher reliability of daughters' reports of fathers' schooling and occupation could be due to sampling error.

Parameter Estimates

Table 7.3 lists the parameter estimates which obtained under Model VII. The estimated reliabilities of sons' retrospective reports of parental status were consistently lower than those for daughters' retrospective reports or those for parents' self-reports. This difference was largest for reports of mothers' education. The estimated reliabilities of daughters' reports of mothers' schooling were only slightly lower than those of parents' reports.

Tests of Parent/Child and Male/Female Differences in Report Reliabilities

As a last step, I tested whether these differences in reliabilities could be due to sampling variability. Model VIII constrained parents and daughters to be equally reliable when reporting mothers' schooling. Model VIII did not provide a

Table 7.3 (Page 1 of 2)

PARAMETER ESTIMATES--MODEL VII

Characteristic	Reporter	True Score Variance	Error Variances	Slopes (Standard Errors are in Parentheses)	Reliability
Fathers' schooling	Father		2.005	1.000	.823
	Son	9.349	2.796	.933 (.050)	.744
	Daughter		2.005	1.000	.823
Fathers' occupation	Father		66.8	1.000	.837
	Son	343.8	88.4	.902 (.048)	.760
	Daughter		68.8	1.000	.837
Mothers' schooling	Father		.834	1.000	.894
	Son	7.015	1.778	.927 (.038)	.772
	Daughter		1.044	.980 (.033)	.866
	Mother		.834	1.000	.894

Table 7.3 (page 2 of 2)
(Estimated covariances in reporting errors)

Between Errors in:	Covariance (Standard Errors in Parentheses)	Correlations
Sons' reports of mothers' and of fathers' schooling	.544 (.201)	.244
Fathers' reports of their own and of wives' schooling	.272 (.101)	.210

significantly worse fit to the data than did Model VII. Young women's retrospective reports of parental status appeared to be as reliable as parents' self-reports. When I further constrained the model so that the reliabilities of reports of fathers' schooling and occupation were equated for fathers and sons (and thus, for sons and daughters), the new model (Model IX) preserved a satisfactory fit to the data. Thus, the observed lower reliabilities of sons might be due to sampling variability. Finally, Model X equated the reliabilities of sons' and parents' reports of mothers' schooling. Model X provided a significantly worse fit to the data than did Model IX. Sons' reports of mothers' schooling were less reliable than were the reports of mothers, fathers, or daughters. Table 7.4 presents the parameter estimates which obtain under Model IX.

Effects of Measurement Error on Estimates of Sex Differences in the Educational Attainment Process

Table 7.5 presents the regressions of an offspring's education in the parental status measures. Estimates were derived given three different assumptions about the measurement error structure: (1) offsprings' reports were without error; (2) Model VII was correct; and (3) Model IX was correct. Results obtained under the assumption that offsprings' reports were without error were quite similar for men and women, but there was very weak evidence that the education of the like-sex parent had more effect than that of the other parent on young adults' level of schooling. This is consistent with results obtained in previous studies of attainment (Treiman and Terrell 1975a, Featherman and Hauser, 1976). When corrections were made for measurement error, parameter estimates changed slightly within equations, and the suggestive evidence of identification

Table 7.4 (page 1 of 2)
PARAMETER ESTIMATES--MODEL VIII

Characteristic	Reporter	True Score Variance	Error Variances	Slopes (Standard Errors are in Parentheses)	Reliability
Fathers' schooling	Father	9.004	2.230	1.000	.801
	Son		2.230	1.000	.801
	Daughter		2.330	1.000	.801
Fathers' occupation	Father	326.1	73.2	1.000	.817
	Son		73.2	1.000	.817
	Daughter		73.2	1.000	.817
Mothers' schooling	Father	6.946	.884	1.000	.887
	Son		1.740	.939 (.038)	.779
	Daughter		.884	1.000	.887
	Mother		.884	1.000	.887

Table 7.4 (page 2 of 2)

Between Errors in:	Covariance (Standard Errors in Parentheses)	Correlations
Sons' reports of mothers' and of fathers' schooling	.453 (.184)	.230
Fathers' reports of their own and of wives' schooling	.301 (.103)	.215

Table 7.5
REGRESSIONS UNCORRECTED AND CORRECTED FOR MEASUREMENT ERROR
(Dependent Variable=Years of Schooling)

Assumptions About Measurement Error	Male Equation			Female Equation		
	Coefficients (Standard error)			Coefficients (Standard error)		
	ED_{f-t}	OCC_{f-t}	ED_{m-t}	ED_{f-t}	OCC_{f-t}	ED_{m-t}
1) Sons' reports are without error	.207** (.065)	.019* (.010)	.140** (.069)	.153** (.068)	.009 (.010)	.230** (.065)
2) Model VII--covariances between fathers' reports of own and of mother's education and between sons' reports of mothers' and fathers' schooling, reliabilities equated for fathers' and daughter's reports of father's traits and for fathers' and mothers' reports of mother's education	.262** (.113)	.014 (.015)	.146* (.087)	.120 (.108)	.013 (.014)	.286** (.085)
3) Model IX--Model VII with reliabilities equated for parents' and daughters' reports of mother's education and for sons' and fathers reports of fathers' traits	.276** (.117)	.014 (.015)	.152* (.088)	.125 (.116)	.013 (.015)	.283** (.087)

with the same-sex parent was strengthened. The impact of an additional year of parental schooling on an offspring's schooling was larger by a factor of two when the parent and offspring were the same sex. Note, however, that corrections for measurement errors had only modest effects within individual equations.

Summary of Findings

These findings may be summarized as follows:

1) Daughters' retrospective reports of fathers' schooling and of fathers' occupation were as reliable as were fathers' self-reports, and daughters' retrospective reports of mothers' schooling were not significantly less reliable than were parents' reports. There was no evidence to suggest that there were non-random errors in young women's retrospective reports.
2) Although the estimated reliabilities of sons' reports of parental education were consistently lower than those estimated for parents' self-reports, these differences were only significant for mothers' education. That is, sons' reports of fathers' schooling and fathers' occupation were not significantly less reliable than fathers' self-reports. This finding is consistent with the work of Bielby et al. Sons' retrospective reports of mothers' schooling were less reliable than either fathers' reports, or mothers' reports, suggesting that the quality of men's retrospective reports of maternal traits may be relatively poor. Errors in sons' reports of mothers' and fathers' schooling were correlated ($r = .23$), suggesting that sons used one parent's schooling to guess about the schooling of the other parent. (Note: if this correlation were due to similarities in questions or coding, then it is surprising that it did not show up for daughters.)
3) The reliabilities of parents' reports of their own or of their spouses' status traits were quite high (.801 to .887) and were consistent with those obtained in other studies. In addition, errors in men's reports of their own and their wives' schooling were correlated ($r = .22$), suggesting that men tended to make reports of their own and of their wives' schooling consistent.
4) There was persuasive evidence that young men's and women's measurement error structures differed. Sons' retrospective reports of mothers' schooling were considerably less reliable than were daughters' retrospective reports. Further, there was no evidence of non-random

error in daughters' reports of parental traits, but the errors in sons' reports of their mothers' and fathers' schooling were correlated.

5) There was suggestive but far from conclusive evidence that not correcting for measurement error may underestimate the extent of sex-role modeling on the same-sex parent in the schooling process. This suggests that it may be important for future researchers to consider the possibility that failure to correct for measurement error may reduce estimates of sex differences in the status attainment process.

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Appendix 7A.1

Table 7A.1 lists the questions asked about parents' status traits for each type of respondent. Although the questions used differed, coding procedures were quite similar. Education could be collapsed into the following eight categories.

1) 0-5 grades
2) 6-8 grades; (grade school)
3) 9-11 grades (some high school)
4) 12 grades (completed high school)
5) 12 grades plus non-academic training; R.N. (no further elaboration)
6) Some college, no degree; Associate's degree
7) College, BA and no advanced degree mentioned; normal school; R.N. with 3 years college; "college"
8) College, advanced or professional degree, some graduate work; close to receiving degree

There was one major difference in coding procedures. Parents who reported they "didn't know" their own (or their spouses') schooling were coded as missing data and so were eliminated from this analysis. Splitoffs who reported they didn't know their parents' schooling were coded in category 2 (6-8 years) if they reported that their parents could read and write. To deal with this, all splitoffs with missing data who were coded as 2's were recoded as missing data (and thus omitted from analysis). In addition, categories 4 and 5 were collapsed into one category (since in both categories respondents reported 12 years of school) and were assigned as midpoints to each category.

Occupations were sight-coded into broad occupation categories by trained occupation coders. Whenever an assignment was unclear, coders assigned the occupation a 3-digit Census occupation code and then assigned it to the relevant one-digit category. One-digit occupational categories were converted to Duncan scores for the purpose of analysis.

Table 7A.1 (page 1 of 3)

QUESTION ASKED ABOUT PARENTAL CHARACTERISTICS

(Reports of parental education.)

1. Questions on father's schooling asked of father in 1967.

M3. How many grades of school did you (HEAD) finish?____________________
(V313)

(IF 6 GRADES OR LESS)

M4. Did you get any other training?
(V314)
☐ YES ☐ NO-(GO TO M6)

M5. What was it?_______________
(V314)
____

M6. Do you have any trouble reading?
____

(IF 7 GRADES OR MORE)

M7. Did you have any other schooling?
(V314)
☐ YES ☐ NO-(GO TO M11)

M8. What other schooling did you have?
(V314)
____

M9. Do you have a college degree?

☐ YES ☐ NO - (GO TO M11)

M10. What degree(s) did you receive?
____

(GO TO M11)

2. Questions on mother's schooling asked of fathers in 1967.

I 16. How many grades of school did your wife finish?____________________
(V246)

I 17. Did she have any other schooling?
(V246)

YES NO (GO TO I 21)

I 18. What other schooling did she have?____________________
(V246)

(IF COLLEGE) I 19. Does she have a college degree?

YES NO (GO TO I 21)

I 20. What degree(s) did she receive?_______________
____

Table 7A.1 (page 2 of 3)

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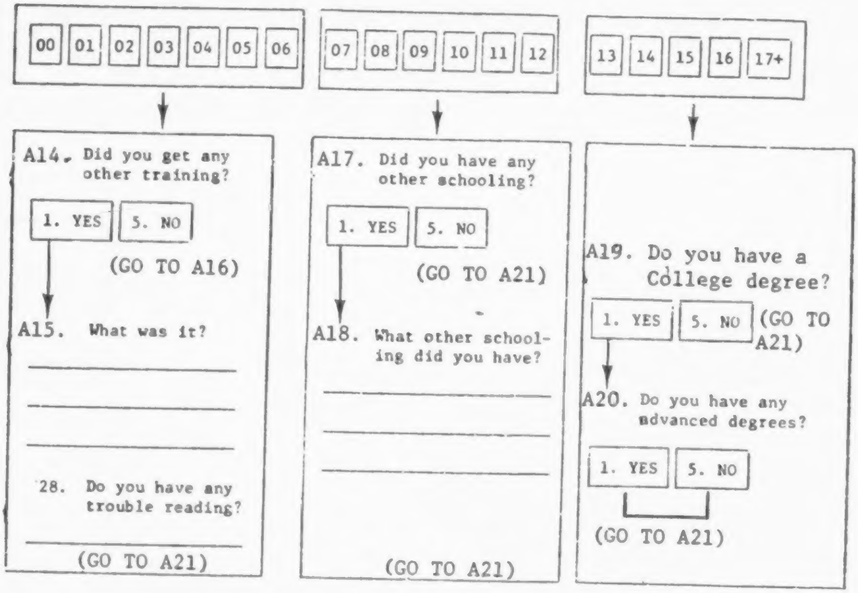
3. Questions on mother's schooling asked of mothers in 1976.

A13. How many grades of school did you finish?



00 01 02 03 04 05 06

07 08 09 10 11 12

13 14 15 16 17+

A14. Did you get any other training?

1. YES 5. NO (GO TO A16)

A15. What was it?

28. Do you have any trouble reading?

(GO TO A21)

A17. Did you have any other schooling?

1. YES 5. NO (GO TO A21)

A18. What other schooling did you have?

(GO TO A21)

A19. Do you have a College degree?

1. YES 5. NO (GO TO A21)

A20. Do you have any advanced degrees?

1. YES 5. NO

(GO TO A21)

Table 7A.1 (page 3 of 3)

4. Questions on father's schooling and mother's schooling asked of sons and daughters.

   K20. How much education did your father have?____________________

   (IF LESS THAN 6 GRADES) K21. Could he read and write?______

   K22. How much education did your mother have?____________________

   (IF LESS THAN 6 GRADES) K23. Could she read and write?_____

5. Questions asked father about his occupation.

   (IF WORKING)
   F2. What is your main occupation? (What sort of work do you do?)

   ____

   (IF NOT CLEAR) F3. Tell me a little more about what you do?________

   ____

   F4. Do you work for someone else, yourself, or what?

   SOMEONE ELSE BOTH SOMEONE ELSE AND SELF SELF ONLY (TURN TO F11)

6. Questions asked sons and daughters about their father's occupation.

   K2. What was your father's usual occupation when you were growing up.

   OCC

   ____

   ____

Appendix 7A.2

This appendix describes in detail the underlying structure of the various measurement models examined in the text. Let the general multiple group measurement model (equations 8 to 14) be represented as:

$$\underline{Y}_k = \underline{\Lambda}_k \underline{T}_k + \underline{\varepsilon}_k$$

where $\underline{Y}$ = 7 x 1 vector of observed parental and offspring reports

$$= \begin{bmatrix} ED_{f-f-k} \\ ED_{f-o-k} \\ OCC_{f-f-k} \\ OCC_{f-o-k} \\ ED_{m-f-k} \\ ED_{m-o-k} \\ ED_{m-m-k} \end{bmatrix}$$

Λ_k = 7 x 3 matrix of coefficients

$$= \begin{bmatrix} 1 & 0 & 0 \\ \lambda_{1ok} & 0 & 0 \\ 0 & 1 & 0 \\ 0 & \lambda_{2ok} & 0 \\ 0 & 0 & \lambda_{3fk} \\ 0 & 0 & \lambda_{3ok} \\ 0 & 0 & 1 \end{bmatrix}$$

$\underline{T}_k$ = 3 x 1 matrix of true scores

$$= \begin{bmatrix} ED_{f-t-k} \\ OCC_{f-t-k} \\ ED_{m-t-k} \end{bmatrix}$$

$\underline{\varepsilon}_k$ = 7 x 1 vector of disturbance terms

$$= \begin{bmatrix} \varepsilon_{1fk} \\ \varepsilon_{1ok} \\ \varepsilon_{2fk} \\ \varepsilon_{2ok} \\ \varepsilon_{3fk} \\ \varepsilon_{3ok} \\ \varepsilon_{3mk} \end{bmatrix}$$

and k = s for sons
        d for daughters

This gives the following covariance matrix of observed scores

$$\Sigma(\underline{Y}_k \underline{Y}_k{}') = \Lambda_k \overline{\underline{\Phi}}_k \Lambda_k{}' + \underline{\Psi}_k$$

where

$\overline{\underline{\Phi}}_k$ = 3 x 3 covariance matrix of true scores

$$= \begin{bmatrix} \mathrm{Var}(ED_{f-t-k}) & & \\ \mathrm{Cov}(OCC_{f-t-k} ED_{f-t-k}) & \mathrm{Var}(OCC_{f-t-k}) & \cdots \\ \mathrm{Cov}(ED_{m-t-k,} ED_{f-t-k}) & \mathrm{Cov}(ED_{m-t-k,} OCC_{f-t-k}) & \mathrm{Var}(ED_{m-t-k}) \end{bmatrix}$$

$\underline{\psi}_k$ = 7 x 7 covariance matrix of error term

$$= \begin{bmatrix} \mathrm{Var}(\varepsilon_{1fk}) & \cdots & & \mathrm{Cov}(\varepsilon_{1fk}, \varepsilon_{3mk}) \\ \mathrm{Cov}(\varepsilon_{1ok}, \varepsilon_{1fk}) & \mathrm{Var}(\varepsilon_{2fk}) & \cdots & \vdots \\ \vdots & & & \\ & & & \\ \mathrm{Cov}(\varepsilon_{3mk}, \varepsilon_{1fk}) & \cdots & & \mathrm{Var}(\varepsilon_{3mk}) \end{bmatrix}$$

In order to specify that the joint distributions of true parental status and the reliabilities of parents' scores did not vary by sex, we must equate the true score covariance matrices for sons and daughters ($\underline{\Phi}_d = \underline{\Phi}_s$) and must equate the error variances of the parents' reports of the parental characteristics for sons and daughters. Equating error variances gives:

$$\mathrm{Var}\ (\varepsilon_{1fd}) = \mathrm{Var}\ (\varepsilon_{1fs});\ \mathrm{Var}\ (\varepsilon_{2fd}) = \mathrm{Var}\ (\varepsilon_{2fs});\ \mathrm{Var}\ (\varepsilon_{3fd}) = \mathrm{Var}\ (\varepsilon_{3fs});$$

$$\mathrm{Var}\ (\varepsilon_{3md}) = \mathrm{Var}\ (\varepsilon_{3mf}).$$

In addition, we must equate the slopes of the regression of true mother's schooling on the father's report of mother's schooling ($\lambda_{3fd} = \lambda_{3fs}$).

This chapter examined ten different forms of the above general measurement model, (Models I to X). How the general model is constrained in Models I to X is described below.

The random error model, Model I, implies that all error covariances are zero. So in Model I, $\underline{\psi}_d$ and $\underline{\psi}_s$ were constrained to be diagonal. Model II allows children's reporting errors to covary. We obtain Model II from Model I by freeing the following elements in $\underline{\Psi}_d$ and in $\underline{\Psi}_s$:

$$\mathrm{Cov}(\varepsilon_{1od}, \varepsilon_{2od});\ \mathrm{Cov}(\varepsilon_{1od}, \varepsilon_{3od});\ \mathrm{Cov}(\varepsilon_{2od}, \varepsilon_{3od});\ \mathrm{Cov}(\varepsilon_{1os}, \varepsilon_{2os});$$

$$\mathrm{Cov}(\varepsilon_{2os}, \varepsilon_{3os});\ \mathrm{Cov}(\varepsilon_{1os}, \varepsilon_{3os}).$$

Model IV is similar to Model II, but also allows errors in fathers' reports of their schooling and of their wives' schooling to covary. Thus Model IV is obtained from Model II by freeing

$$\mathrm{Cov}(\varepsilon_{1fd}, \varepsilon_{3fd}) \text{ and } \mathrm{Cov}(\varepsilon_{1fs}, \varepsilon_{3fs}).$$

Also, since we assume that parents' measurement error structures do not vary by sex of child we must constrain these covariances to be equal

$$\mathrm{Cov}(\varepsilon_{1fs}, \varepsilon_{3fs}) = \mathrm{Cov}(\varepsilon_{1fd}, \varepsilon_{3fd}).$$

Model V is very similar to model IV--except that we assume the measurement structures do not vary by sex. Thus, Model V sets $\Lambda_s = \Lambda_d$, and $\underline{\Psi}_d = \underline{\Psi}_s$.

Model VI sets all insignificant error covariances from Model IV equal to zero:

$$\mathrm{Cov}(\varepsilon_{1os}, \varepsilon_{2os}) = \mathrm{Cov}(\varepsilon_{2os}, \varepsilon_{3os}) =$$

$$\mathrm{Cov}(\varepsilon_{1od,3od}) = \mathrm{Cov}(\varepsilon_{1od}, \varepsilon_{2od}) = \mathrm{Cov}(\varepsilon_{2od}, \varepsilon_{3od}) = 0$$

Model VII is obtained from Model VI by constraining the reliabilities of fathers' and daughters' reports of fathers' schooling to be equal, by constraining the reliabilities of fathers' and daughters' reports of fathers' occupation to be equal, and by constraining the reliabilities of fathers' and mothers' reports of mothers' schooling to be equal. Relevant slopes and error variances were equated as follows:

$$\lambda_{1od} = \lambda_{1fd} = 1.0;\ \mathrm{Var}\ (\varepsilon_{1od}) = \mathrm{Var}(\varepsilon_{1fd})$$

$$\lambda_{2od} = \lambda_{2fd} = 1.0;\ \mathrm{Var}\ (\varepsilon_{2od}) = \mathrm{Var}(\varepsilon_{2fd})$$

$$\lambda_{3fd} = \lambda_{3fs} = \lambda_{3md} = \lambda_{3ms} = 1.0;\ \mathrm{Var}(\varepsilon_{3fd}) = \mathrm{Var}(\varepsilon_{3fs}) = \mathrm{Var}(\varepsilon_{3md}) = \mathrm{Var}(\varepsilon_{3ms})$$

Model VIII constrains Model VII so that the reliabilities of daughters' reports of mother's schooling equal those of fathers' and mothers' reports.

$$\lambda_{3od} = \lambda_{3fd} = \lambda_{3md};\ \mathrm{Var}(\varepsilon_{3od}) = \mathrm{Var}(\varepsilon_{3fd}) = \mathrm{Var}(\varepsilon_{3md}).$$

Model IX equates the reliabilities of sons' reports of father's traits to the reliabilities of daughters' and of fathers' reports.

$$\lambda_{1os} = \lambda_{1od} = \lambda_{1fs};\ Var(\varepsilon_{1os}) = Var(\varepsilon_{1od}) = Var(\varepsilon_{1fs})$$

$$\lambda_{2os} = \lambda_{2od} = \lambda_{2fs};\ Var(\varepsilon_{2os}) = Var(\varepsilon_{2od}) = Var(\varepsilon_{2fd})$$

Model X is obtained from Model IX by equating the reliabilities of sons' reports of mother's schooling to those of mothers', fathers' and daughters' reports.

$$\lambda_{3os} = \lambda_{3ms} = \lambda_{3fs} = \lambda_{3od}\ Var(\varepsilon_{3os}) = Var(\varepsilon_{3ms}) = Var(\varepsilon_{3fs}) = Var(\varepsilon_{3od}).$$

Chapter 8

OCCUPATIONAL DISABILITY AND ITS ECONOMIC CORRELATES

James N. Morgan

INTRODUCTION

The onset of a disability that limits the type or amount of work a person can do can be thought of as occurring each year to some subset of the total population. Disability would send some individuals rapidly to the grave, or to a nursing home or other institution and, thus, they would disappear from samples of the non-institutionalized population. Disabled persons tend to be overlooked in most studies, too, when they move to the homes of relatives, or move to the category of "wives not working for money". In this analysis we provide some information on disabled family members other than the family head, but our main focus is on the impact of disability on those individuals, including single people, who remain heads of families. It is necessary in this discussion to remember that we are talking about only part of the economic impact of disability.

It will become apparent that self-descriptions of disability are sensitive to the method of eliciting information, not so much in the total numbers who mention some disability, but in their own characterization of its severity. Since the economic impact is concentrated among those with severe disabilities, the distinction is important.

Estimates of the economic impact also depend on the method, particularly on whether we use only the non-disabled in setting the normal expectations with which we compare the disabled, rather than treating disability as one variable in a multivariate analysis.

Static cross-section estimates are insufficient for this type of study, particularly if there are lags and adaptations to disability or if we think people may give up, quit, and then describe themselves as disabled. So we present some more dynamic data on the effect of initial disability or its onset on changes in work, earnings, and transfer incomes.

Definitions

Disability is difficult to define clearly, or to measure precisely, but it is an important phenomenon nonetheless. We can define medical disability in terms of diagnosis, such as diabetes, or hypertension. Or we can define functional disability in terms of capacity to get up and down stairs unaided, or take care of oneself without help. Or we can define a kind of occupational disability as any physical or emotional restriction on the type or amount of work a person can do. Such a definition really reflects an interaction between the functional limitations of the person and the demands of the work and environment. There are people who can earn a good living while immobilized, using the telephone and other aids. But a typist, for example, needs both hands and good eyes and usually the ability to get to where the work is.

A recent discussion paper used the 1977 Current Population Survey to study the impact of disability and identified the disabled not by direct question but by reports of income from workmen's compensation, veterans' compensation, or (for those under 64) from SSI or Social Security, by reported work limitations as reasons for not working the previous year or the previous week, and by reported wages under $1.00 and work in an occupation that allowed sheltered workshops (Wolfe, 1979).

We report here some data on self-reports of occupational disability, at more than one point in time for surviving panel members, and on the relation of such reports to work, to earnings, and to transfer incomes that make up part of the loss. The panel reinterviews have allowed us to see the impacts of initial disability and of changes in reported disability on subsequent changes in earnings. The effects were substantial, and the economic burden was only partially offset by larger transfer incomes.

The Data

In the eleventh wave of the Panel Study of Income Dynamics, an expanded sequence of questions was asked about disability. These questions focused on occupational disability, rather than on medical problems or functional limitations:

"Do you (HOUSEHOLD HEAD) have any physical or nervous condition that limits the type of work or the amount of work you can do?"

IF YES:

"Does it limit your work a lot, somewhat, or just a little?"

"How long have you had this condition?"

"Do you expect it to get better, worse, or stay about the same?"

"Do you require a lot of extra care by someone?"

IF YES

"Does that mean extra costs for the family?"

IF YES

"Are those costs small, moderate, or quite large?"

A similar sequence was asked for others in the family 18 years of age or older. For others in the family under 18 the same questions of detail followed this revised introductory question:

"Is there anyone under 18 in this family who has any physical or nervous condition that limits their activity or schooling?"

Reliability of the Disability Reports

Questions focused on even a single kind of disability (here, the kind of disability that limits the kind or amount of work one can do) are subjective reports likely to be imprecise. In earlier years, people frequently reported a disability they had had for for several years when they had not reported any disability in the prior interviews. However, the fact that reports of current disability in 1972 and 1976 were often followed by dropping out of the labor force lends some credence to the accuracy of the report.

Two other recent studies seem to indicate that when people claim they are disabled, they are. The first of these studies concluded that there was little evidence that the earnings limit for disability benefits under the Social Security system is a work disincentive. In particular, neither the increase in the allowable earnings from $140 a month to $200 a month in 1974, nor earlier increases in 1966 and 1968, appeared to have been followed by incremental increases in beneficiary earnings (Franklin and Hennessey, 1979).

The second study, a Danish study of applicants for disability pensions in 1971, followed for five years the experiences of both persons who were awarded pensions and those who were refused such pensions. Thirteen percent of those who were awarded pensions after their first applications and 28 percent of those whose applications were refused were engaged in gainful employment at the time of interviewing--a small difference. The author concluded:

> The slight attachment to the labour market of those who were refused a pension shows that largely these have not had any occupational function in spite of the fact that the Invalidity Insurance Court assessed them to have an employment capacity of more than 50 percent of normal capacity. The decisions of the court, based on the disability concept as laid down

> in the Disability Pensions Act, thus correspond only in part to the actual course of employment in the subsequent five years.
>
> The implication is that self-report of disability may be a better predictor of later work than a medical examination (Hubbe, 1979).

FINDINGS

We focus here on disabilities of the family head, since they are most likely to affect the family economic status, and we look at the relationship of disability to whether the head had worked, to work hours, to wage rate, to whether there was transfer income, and to the amount of transfer income.

We also examine the effect of changes in disability from 1972 to 1978 and from 1976 to 1978 on change in work and in economic status.

These questions were asked even of those already retired, since the term "work" can include more than just paid employment. There was, of course, a strong age difference in reported disabilities. Table 8.1 shows that the proportion who were disabled rose from less than a tenth of those under the age of 35 to more than half of those 72 or older. The age categories were chosen to separate groups at ages where retirement is possible (age 62), where retirement is expected (age 65), or where Social Security benefits are not affected by earned income (age 72).

It is, of course, a possibility that people can quit working and then later notice disabilities that would impair the amount or type of work they could do. More important, age itself has a powerful effect on work, earnings, and transfer incomes, so that the effect of disability must be adjusted to remove age effects if we are to avoid spurious correlations.

Before turning to our inferences about the economic impact of disability on work, earnings, and transfers, however, it is instructive to see how age correlates with answers to other questions about disability. Table 8.2 shows that older people not only were more likely to report a disability, they were also more likely to report that it limited work a lot and that the disability was staying the same or getting worse. Very few admitted to disabilities requiring a lot of extra care, and still fewer said that such care involved extra costs for the family. For the ten percent of those 72 or older who reported such need and the 4 percent who reported extra costs, however, a serious economic impact existed.

Table 8.2 also shows that a substantial minority, particularly of those 62 or older, had one or more other adults in the family who were disabled, usually

Table 8.1

SELF-REPORTED OCCUPATIONAL (WORK) DISABILITY BY AGE
(For all 6154 Household Heads in 1978)

	Disability					
Age	No Disability	A Lot	Somewhat*	Just A Little	Total	N
< 35	92.5	1.8	2.3	3.4	100	2791
35-44	90.5	4.5	1.4	3.6	100	888
45-54	81.6	10.0	4.7	3.7	100	943
55-61	68.8	19.4	8.5	3.2	100	553
62-64	65.2	15.6	13.6	5.6	100	204
65-71	59.1	26.5	8.8	5.5	100	4.9
72 +	44.9	38.0	10.7	6.4	100	366
Total	78.7	12.1	5.2	4.1	100	6154

*NA's on how much limited are included in "somewhat" (0.3%).

the wife. A small minority of families, mostly headed by younger persons, reported a disabled child under 18. These data include families headed by a single adult, with or without anyone else present, and Table 8.3 shows that single women--who are mostly older women--reported the most extensive disability. It is clear that studies of the impact of disability should adjust for sex and marital status as well as for age. Since earnings are heavily affected by education, the later should also be controlled for.

Before we look at two components of work hours (average hours for those who worked and the proportion who worked), it is useful to consider the effects of age and disability on work hours for both workers and non-workers. The results of regression adjustments using categorical predictors on expected work hours by age and by extent of disability are shown in Table 8.4.[1]

The apparent effects of age and disability are attenuated when considered simultaneously in a regression that includes both and also includes education, sex-marital status, and whether there were other disabled people in the family.

---

[1] It should be noted that there is also a small possibility of a disability happening early in 1978, without affecting work during 1977.

Table 8.2

DISABILITY OF HUSBAND OR SINGLE HEAD OR OTHERS, BY AGE OF HEAD

	Disability of Husband or Single Head						Others Disabled in Family		
Age	A Disability	Limits Work A Lot	Stay Same or Getting Worse	Requires Extra Care	Involves Extra Costs	Costs Are Large	Aged >17	Aged <18	N
<35	7	2	6	1	0	0	2	2	2791
35-44	9	5	7	1	1	1	5	4	888
45-54	18	10	14	2	1	0	9	4	943
55-61	31	19	26	4	3	1	12	1	553
62-64	35	16	31	1	1	1	18	0	204
65-71	41	27	32	4	1	1	17	1	409
72+	55	38	48	10	4	2	17	0	366
All Ages	21	12	17	3	1	1	8	2	6154

Note: Columns are not mutually exclusive

Table 8.3

DISABILITY OF HUSBAND OR SINGLE HEAD, BY STATUS

	Disability of Husband or Single Head						Others Disabled in Family		
Sex-Marital Status	Any Disability	Limits Work A Lot	Staying Same or Getting Worse	Requires Extra Care	Involves Extra Care	Costs Are Large	Age >17	Aged <18	N
Single man	22	12	17	4	2	0	3	1	813
Single woman	30	18	25	3	1	1	3	2	1732
Married man	17	9	14	2	1	1	12	2	3589
All	21	12	17	3	1	1	8	2	6154

Note: Columns are not mutually exclusive

Table 8.4

HOURS WORKED BY AGE, DISABILITY, AND SEX-MARITAL STATUS
(For all household heads including those not working)

Explanatory Characteristic	Percent of Sample	Hours Worked in 1977	
		Unadjusted	Adjusted
Age			
<34	35	1932	1809
35-44	14	2091	1939
45-54	17	1952	1862
55-61	10	1567	1636
62-64	4	1249	1393
65-71	9	422	660
72+	11	134	544
Eta^2, $Beta^2$		.39	.21
How Much Limited			
Not disabled	79	1813	1681
Just a little	4	1171	1338
Somewhat	5	851	1267
A lot	12	312	939
Eta^2, $Beta^2$		.23	.05
Sex-Marital Status			
Married man	58	1862	1760
Single woman	28	922	1159
Single man	14	1574	1517
Eta^2, $Beta^2$		.14	.06

The effects of education and the number of other disabled family members are not shown in Table 8.4 since they were insignificant, at least when accounting for the three variables that mattered most (See Appendix Table 8A.1.)

The results of six regressions, each seeking the net effect of disability on a component of economic status adjusted for age and the other characteristics, are summarized in Table 8.5. The model behind the estimation is that disability (considered an exogeneous event) affects whether the individual works, the work hours of those who do work, and perhaps the wage rate of those who work. It also affects whether there is transfer income and, if so, how much. A more elaborate model would allow the effects on work and earnings to affect transfer income, with disability at least in part working its effect on transfers through its effect on earnings (income test). Here, however, we are more interested in the final results--the impact of disability on economic status after transfers have offset part of the income loss.

It is clear from the results shown in Table 8.5 that disability had a powerful effect on whether people worked, apart from their age, and on their work hours, but not on how much they made per hour if they worked. It also had a powerful effect on whether they received any transfer income but a much weaker effect on the amount of transfer income largely independent of the extent of the disability.

We can multiply across to get the total adjusted effects on earned income and on transfer income, and add those two to get the expected effects of disability on income in general, assuming that other forms of income (e.g., from assets) are unaffected. Table 8.6 gives these results. The last column indicates the expected earned-plus-transfer income for each of the groups, adjusted for their differences in age, sex-marital status, education, and the presence of other disabled family members. Even when transfers offset some of the impact, there was a \$3,000 to \$5,000 a year difference in the family head's income associated with his or her disability. Readers who like aggregate figures can multiply these expected impacts by the number of such disabled people in the population, which is the product of the fraction of the sample so disabled times the number of households in the United States (76 million in 1978).

Change in Disability

Dynamic inferences from cross-section correlations are always dangerous, so it is useful to look at the relationship between existence and change in disability on the one hand, and change in work, earnings, and transfer incomes on the other (Vincente, Wiley, and Carrington, 1979). We have done this for two time periods--1978 reports combined with 1972 reports and with 1976 reports (that is, five-year and two-year changes). However, although the question about disability was unchanged, the question on severity was changed from the open-ended "How much does it limit your work?" to the fixed-alternative: "Does it limit your work a lot, somewhat, or just a little?"

Table 8.5

EXPLANATORY POWER OF FIVE CHARACTERISTICS INCLUDING DISABILITY* AND ESTIMATED EFFECTS

Explanatory Power

Characteristic	No. of Classes	Whether Works		If Worked: Wage Rate		If Worked: Work Hours		Family Income/Needs		Whether Transfer Income		If Transfers: Amount	
		Gross Eta^2	Net $Beta^2$	Gross Eta^2	Net $Beta^2$	Gross Eta^2	Net $Beta^2$	Gross Eta^2	Net $Beta^2$	Gross Eta^2	Net $Beta^2$	Gross Eta^2	Net $Beta^2$
Age	7	.45	.26	.04	.05	.12	.09	.05	.07	.23	.12	.18	.17
Education	10	.14	.01	.07	.09	.02	.01	.17	.16	.10	.02	.04	.02
Sex-marital	5	.09	.02	.04	.03	.08	.07	.08	.05	.08	.03	.02	.02
How much disabled	4	.27	.08	.00	.00	.05	.02	.05	.02	.13	.03	.05	.01
If others disabled	4	.01	.00	.00	.00	.01	.00	.01	.00	.01	.00	.03	.01
R^2 adjusted			.55		.15		.22		.29		.32		.22

Unadjusted and Adjusted Effects of Disability

Extent of Disability	Percent of Sample	Unadj	Adj	Unadj	Adj	Unadj	Adj	Unadj	Adj	Unadj	Adj	Unadj	Adj
No disability	78.7	.88	.83	$6.96	$6.91	2050	2032	5.84	5.66	.42	.46	2697	2901
Just a little	4.1	.65	.71	5.87	6.06	1797	1826	4.87	5.25	.67	.60	3653	3434
Somewhat	5.2	.56	.71	6.31	6.37	1512	1645	4.88	5.32	.79	.65	3608	3291
A lot	12.1	.24	.49	5.59	6.42	1280	1557	3.10	4.00	.92	.70	3909	3482
	100.0												
N			6154		4959		4959		6154		6154		2879

*Using regressions with categorical predictors

Table 8.6

DISABILITY REPORTED IN 1978, EFFECTS ON 1977 WORK
EARNINGS, TRANSFER INCOMES, NET REGRESSION-ADJUSTED EFFECTS
(For all 6154 heads of early 1978*)

		If Worked							
Extent of Disability	Percent of Sample	(1) Whether Disabled	(2) Hours	(3) Wages	(4) Whether Transfers	(5) Amt. of Transfers (If Any)	1x2x3=6 Expected Annual Earnings (Adj.)	4x5=7 Expected Transfer (Adj.)	6x7=8 Expected Earnings + Transfers
None	79	.83	2032	$6.91	.46	$2901	$11,654	$1334	$12,988
Limits just A little	4	.72	1826	6.06	.60	3434	7,857	2060	9,917
Somewhat	5	.71	1645	6.37	.65	3291	7,440	2139	9,579
A Lot	12	.49	1557	6.42	.70	3482	4,898	2437	7,335
B^2		.077	.021	.001	.030	.012			
Mean		.78	1993	$6.84	.50	$3089			
R^2 Adj.		.554	.222	.150	.324	.333			
N		6154	4959	4959	6154	2879			

*Adjusted for age, sex-marital status, education, and number of other disabled in household.

The overall distributions of replies indicate that the controlled central-office coding gave different distributions. For the full sample for each year, we have:

1972 and 1975 Disability Codes:	Central Office Coding 1972	Central Office Coding 1976	1978	1978 Categories (Limits work-)
Completely ("I can't work.")	6.1	5.1	12.1	A Lot
Severely ("It limits me a lot")	4.1	3.1		
"Some," "not much," "can only work a few hours at a time," "must rest," "can't lift heavy objects," reports periods of pain	8.3	6.4	4.8 4.1	Somewhat Just a little
Limitation but not on work	1.2	0.6	No such category	
Not ascertained, don't know	2.1	3.2	0.3	N.A., D.K.
No disability	78.2	81.6	78.7	No disability

Using pairs of reports, we formed the following combination categories on disability and we used all eight categories in regression analysis of change in work, earnings, and transfer incomes.

	1978 Disability			
1972 or 1976 Disability	A Lot	Somewhat	Just a Little	Not Disabled (or NA how severe)
Completely or severely	7. Originally "completely" or "severely" disabled 1.9%			
Some	1. Became disabled "a lot" 3.7%	3. Became somewhat disabled 3.2%	8. Orginally somewhat disabled, got no worse 3.0%	
All others			5. Became just a little disabled 2.5%	9. Never disabled 85.7%

And we used all eight categories in regression analyses of change in work, earnings, and transfer incomes.

Change in Disability and Change in Economic Status 1976 to 1978

When the effects of patterns of disability and change in disability are adjusted for the effects of age, education, sex-marital status, and the presence of other disabled people in the household, we can estimate the net effects as shown in Table 8.7. When changes are involved we cannot multiply through probabilities of working, hours, and wage rates, because those who retire have a change in hours (and wage rate) to zero, separately from changes of those who continue working. Since the main effect in any case is on retiring rather than on the hours or wage rates of those who continue working, we included in this analysis all the (constant) family heads who were initially working and looked at increasingly comprehensive measures of change: retiring, change in hours and earnings, (including changes to zero), change in annual earnings, and change in transfer earnings. The last two were finally added to get an expected change in the family heads income, though some wives' transfers may be included. The main effects, of course, are on dropping out of the labor force entirely and receiving increased transfer incomes. The adjusted expected changes in hours are larger than those given if we look only at those who stayed in the labor force. The change in hourly earnings for those still working hardly differed from those with no disability or change in disability. But the combination of important changes in labor force participation and smaller changes in hours for some persons still working resulted in much more unfavorable changes in annual earnings for those who became disabled or for those who were disabled in the first place.

Larger increases in transfer incomes, however, offset some of these differences, leaving relative differences in income increases of more than $2000 a year for those who became severely disabled ($2519-$483) and of nearly $1300 for those who became somewhat disabled, but of less than $200 for those who became "a little" disabled.

Except for those who started with severe disabilities, the economic impact of disability was only fractionally offset by changes in transfer income. Those who became seriously disabled, for instance, experienced a $1250 loss in earnings while those never disabled had income gains of $2317, which netted the disabled a comparative earnings loss of $3567 (with figures adjusted for age, etc.). But their increase in transfer income was only $1532 larger than for those never disabled $1733-$202, offsetting less than half the earnings loss. For those who became "somewhat" disabled, transfers offset less than a third of the earnings impact. Those who started out disabled suffered a subsequent earnings deficiency of $2649 compared to the "never disabled," but they experienced a large relative

Table 8.7

ADJUSTED EFFECTS OF CHANGING DISABILITY 1976 TO 1978*
(For 3662 constant family heads who worked at least 500 hours in 1975)

Change in Disability	Percent	Whether Retired	Change in Hours/Yr.	Change in Hourly Earnings	Change in Annual Earnings	Change in Transfer Income	Expected Change in (Earnings+ Transfers)
1. Became Severely disabled	3.7	.26	-492	\$0.23	-\$1250	\$1733	\$483
3. Became somewhat disabled	3.2	.04	-333	0.85	405	819	1224
5. Became a little disabled	2.5	.03	-213	1.49	2167	177	2344
7. Originally completely or severely disabled	1.9	.37	-521	-1.33	-332	2144	1812
8. Originally a little disabled got no worse	3.0	.00	85	1.14	2490	104	1594
9. Never disabled	85.7	.03	-26	1.15	2317	202	2519
Beta^2		.10	.03	.01	.02	.04	
R^2 Adj.		.216	.098	.013	.087	.133	

*Adjusted by regression for differences in age, sex-marital status, education, and number of others disabled. In current dollars, not adjusted for inflation.

gain in transfer income of $1942, presumably because much of their earnings loss had already taken place.

Change from 1972 to 1978

A similar analysis of change over a longer period, starting with those who worked at least 500 hours in 1971, produced the estimates shown in Table 8.8 which indicate the net effects of disability and its changes after taking account of the fact that older people are more vulnerable to disability. Again the main effect was through forcing retirement from work, and the net differences from those who were never disabled were larger than for the shorter period, closer to the cross-section estimates. Remember, however, that these figures are unadjusted for inflation; hence, these values exaggerate the real differences.

Those never disabled increased their annual earnings by $4767 and their transfer incomes by $883 (adjusted for age), while those who became disabled "a lot" averaged a $420 reduction in annual earnings, offset by only a $2720 increase in transfer income. Thus, those never disabled averaged an increase in earnings-plus-transfers of $5650, while those who became disabled "a lot" averaged an increase of only $2300.

Two-Stage Estimates Using Norms for the Non-Disabled

Walter Oi, in an important paper entitled Three Paths from Disability to Poverty, argued that the results of simultaneous estimates using dummy variables like those just presented are potentially biased and that instead one should establish expectations on the basis of the non-disabled, then apply them to the disabled to estimate the effect (Oi, 1978). The same procedure has been used in the treatment of the effects of poor health and of racial or sexual discrimination (Luft, 1975; Blender, 1973; Oaxaca, 1973; Corcoran and Duncan, 1978). Oi also pointed out that disability costs people some enjoyable leisure that is also ignored in our dollar estimates of earnings loss, and that there are some psychic costs of poor health.

We started with the cross-section estimates using disability reports in 1978 and simultaneous reports on work and earnings in 1977. We first ran dummy-variable regressions for the non-disabled on whether they worked and, for those non-disabled who worked, on hours and hourly earnings using age, education, sex-and-marital status, and race but not disability, as predictors. The estimates of the power of the predictors and the gross and net age effects are given in Appendix Table A8.4.

Table 8.8

1972 to 1978 CHANGE IN DISABILITY OF HEAD-ECONOMIC IMPACT ADJUSTED BY REGRESSION*

(For 2941 constant family heads who worked at least 500 hours in 1971)

Change in Disability	Whether Retired	Change in Work Hours	Change in Annual Earnings	Change in Transfer Income	Change in Annual Earnings +Transfers	Change in Income/Needs Decile Position
Became disabled a lot	.48	-1045	-$420	$2720	$2300	-.96
Became disabled some	.19	-559	3088	1606	7694	-.01
Became disabled a little	.14	-538	2593	1399	3992	-.16
Originally completely or severely disabled	.34	-481	1303	1695	2998	-.47
Originally a little disabled got no worse	.14	-255	3465	1582	5047	.27
Never disabled	.08	-240	4767	883	5650	.09
Beta^2	.11	.05	.02	.03		.02
N	2941	2941	2941	2941	2941	2941

*Regression adjusts for differences in age, sex-marital status, education, and number of others disabled in the household. Not adjusted for inflation.

The estimated effects of age, education, race, and sex-marital status for the non-disabled were then applied case by case to the characteristics of the disabled to estimate the probability of working and, if working, the expected wage rate and work hours. Deviations of actuality from these expectations ("residuals") were then analyzed in a second dummy-variable regression using the same predictors plus the extent of disability. Appendix Table A8.5 summarizes the power of the predictors and the age effects. The estimates of concern here--the effects of extent of disability--are given in Table 8.9.

A comparison of Tables 8.5 and 8.9 shows that the estimated effects were very similar. A disability that affected work a lot reduced the probability of working by 34 percent according to Table 8.5 by 37 percent according to Table 8.9; it reduced the expected wage by \$.49 in Table 8.5 and by \$.44 in Table 8.9; and it reduced the expected hours of workers by 475 in Table 8.5 and by 473 in Table 8.9. Of course, one could argue for the unadjusted (gross) effects of disability on the residuals, on the grounds that adjusting them for age allows some disability effects to be credited to age. The alternative interpretation is that the age pattern of the residuals indicates an interaction effect--disability affecting different age groups differently, or age affecting the disabled differently from the non-disabled. In this case there is no point in arguing bias, nor can we even specify which way the interaction works. In any case, the effect is small--middle-aged disabled people appeared more likely to quit working (or perhaps middle-aged workers were less likely to report themselves as disabled), and young disabled persons reduced their hours more while continuing to work. (See Table A8.5).

On the other hand, a comparison of the estimated expected work and earnings of the disabled and non-disabled shown in Table 8.6 with those implied by Table 8.9 indicates systematic differences in the expected earnings loss (Table 8.10). Earnings losses appear to be about 10 percent larger with the two-step method that bases norms on the non-disabled rather than on the entire sample. The standard statistical discussion of two-step procedures that analyze residuals points out that the estimates of effects at the second stage will be biased downward to the extent that the variables in the second analysis are correlated with the variables used in the first step. But that happens when the whole sample is used both times. What we have here is a reverse problem--estimates of the normal effects of the other variables, like age, on work and earnings can be interpreted as biased downward if the disabled are left in the sample for those estimates.

Table 8.9

RESIDUALS FROM EXPECTED WORK AND EARNINGS FOR THE DISABLED* (Unadjusted and Adjusted**)

How much Disability Affects Work:	Whether Worked		If Worked: Hourly Earnings		If Worked: Hours	
	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.
Just a little	-.11	-.12	$-.92	$-.95	-203	-192
Somewhat	-.12	-.11	-.60	-.77	-389	-397
A lot	-.33	-.37	-.64	-.44	.471	.473
All disabled	-.24		-.71		-360	
All disabled	-.24		-.71		-360	
R^2 adjusted=		.160		.036		.058
N=	1278	1278	542	542	542	542

*Expected on basis of relations for non-disabled, using age, education, race, sex-marital status of disabled.
**Adjusted by multiple regression including age, education, race, sex-marital status.

Table 8.10

COMPARISON OF EXPECTED WORK, EARNINGS, AND DISABILITY LOSS ESTIMATED BY TWO METHODS

Extent of Disability	Probability of Working	If Worked: Expected Hours	If Worked: Expected Wage	Expected Annual Earnings	Disability Earnings Loss	Two-Step Loss-Estimate Larger by:
One-Step Regression-Adjusted						
Not disabled	.83	2032	$6.91	$11,654	-	
Disabled just a little	.71	1826	6.06	7,857	$3797	-
Somewhat	.71	1645	6.37	7,440	4,214	-
A lot	.49	1557	6.42	4,898	6,756	-
Two-Step (Regression Adjusted Residuals from Non-Disabled Regression Estimates)						
Not disabled	.88	2050	$6.96	$12,556	-	-
Disabled just a little	.76	1858	6.01	8,487	4,069	$272
Somewhat	.77	1653	6.19	7,879	4,687	473
A lot	.51	1577	6.52	6,760	7,322	566

Changes from 1972 to 1978--Two Step Analysis

It is more difficult to decompose the components of the effect of disability when changes are involved, and inflation is more of a problem. Nonetheless, we used the same two-step procedure, using the increasingly comprehensive measures of economic impact as in Table 8.7 (those who retired were included in the estimated changes of hours and earnings). The base-regressions are shown in Appendix Table A8.6, and the regressions on the residuals and the age-effects on the residuals are given in Appendix Table A8.7. All the correlations on change were much lower, of course. The age effects on the residuals were smaller, more erratic, and harder to interpret.

Table 8.11 indicates the two-step estimates of effects of disability and changes in disability on retiring, change in earnings, change in transfer income, and in a global measure, change in family income/needs decile. Inflation is a problem in analysis of such longer periods, hence the change in annual earnings is given as a deflated percentage change. The income/needs decile being a relative position, is also unaffected by inflation. The results were as expected and were more like those from the one-step regression analysis estimates because the other characteristics had so little effect. Initial disabilities were about as influential as those which occurred during the period between 1972 and 1978.

For comparison with the one-step analysis, referring back to Table 8.8 we note that the difference between being never disabled and becoming disabled "a lot" was shown to be 1.05 decile points (from +.09 to -.96) in a one-step procedure and 1.18 decile points (-1.02 compared with +.16 for the never disabled) in the two-step method. (The categories were slightly changed for the initially disabled.) Again, the use of norms for the never disabled gave somewhat larger estimates of the effect of disability.

Perhaps the most important finding is that neither transfer incomes nor other sources of income, such as other family members going to work, substantially offset the disability losses of family heads. Those who became severely disabled saw their families drop a whole decile in income/needs, and those who were initially disabled and did not recover dropped .7 decile.

Disability among the Family Members as a Whole

There is some tendency for disability to cumulate, partly because it is correlated with age, and if one older person is disabled the spouse may also be. Table 8.12 shows the relation between the family head's disability and the number of other family members who were disabled.

Table 8.11 (page 1 of 2)

DEVIATIONS FROM EXPECTATIONS[1] BASED ON THOSE NOT DISABLED IN
EITHER 1972 OR 1978, UNADJUSTED AND ADJUSTED BY REGRESSION
(For 697 who were family heads 1971-1978 and disabled in 1972 or 1978 or both.)

Change in Disability Status		Proportion Retiring		Change in Hours of Work (Including Retirees)		Deflated Percent Change in Annual Earnings (Including Retirees)		Change in Transfer Income (Undeflated)		Change in Income/Needs Decile	
	N	Unadj	Ajd	Unadj	Adj	Unadj	Adj	Unadj	Adj	Unadj	Adj
Became disabled a lot	247	.42	.39	-766	-797	-.62	-.61	$1574	$1841	-1.05	-1.02
Became somewhat disabled	121	.13	.12	-303	-258	-.14	-.13	580	396	-.91	-.11
Became "A little" disabled	92	.06	.10	-277	-245	-.18	-.18	412	143	-.31	-.20
Originally mildly disabled, getting worse	81	.07	.09	+12	+11	+.03	.04	574	442	+.17	+.18

Table 8.11 (page 2 of 2)

DEVIATIONS FROM EXPECTATIONS[1] BASED ON THOSE NOT DISABLED IN EITHER 1972 OR 1978, UNADJUSTED AND ADJUSTED BY REGRESSION

(For 697 who were family heads 1971-1978 and disabled in 1972 or 1978 or both.)

Change in Disability Status	N	Proportion Retiring		Change in Hours of Work (Including Retirees)		Deflated Percent Change in Annual Earnings (Including) Retirees)		Change in Transfer Income (Undeflated)		Change in Income/Needs Decile	
		Unadj	Ajd	Unadj	Adj	Unadj	Adj	Unadj	Adj	Unadj	Adj
Originally completely of severely disabled still at least a little disabled	116	.28	.26	-205	-264	-.20	-.28	530	893	-.49	-.70
Originally completely or severely disabled not disabled now	40	.08	.12	+13	+94	+.01	+.06	-44	-668	+.04	+.08
All		.22[2]		-362[3]		-.27[4]		+844[5]		-.42[6]	

Note: Some in groups 1 and 3 were mildly disabled in 1971 but got worse.

[1] Expectations based on regression parameters estimated using only "never disabled."

[2] Mean deviation .22 means that any disability increased likelihood of retiring (adjusted for age, education, race, sex, marital status) up by 22 percent (from the 5.7 percent of non-disabled).

[3] Non-disabled had a mean change in work hours of -196; those with any disability experience differed from that by a decrease of -362 hours, for a total of -542 hours.

[4] Non-disabled had a 24 percent increase in deflated earnings; disabled, adjusted for age, etc., were 27 percent below this overall for an expected decline of 4 percent in real annual earnings.

[5] Non-disabled had a mean $744 increase in transfer incomes because this sample of "Same heads 1971 to 1978," has more who retire than average and fewer new families. Disabled, adjusted for age, etc., had $844 larger increase in transfer incomes (undeflated).

[6] Non-disabled had a change in decile position of +6.16. Disabled (adjusted for age, etc.) were 0.42 below this—a decrease of 0.36.

Table 8.12

OTHER DISABLED, BY HEAD'S DISABILITY

			Head Disabled Disability Limits Head-		
Others in Family Disabled	Head Not Disabled	Just a Little	Somewhat	A Lot	All
None	93%	79%	77%	80%	90%
One	7	20	21	18	9
Two or more	0	1	2	99	1
	100	100	100	99	100
Percent of sample	79	4	5	12	100

Finally, a series of tables show the impact of disability and its nature among male family heads (single or married), females who were single heads or wives, other males, and other females (by age).

Since the family head was usually the respondent, reports of disabilities of others in the family were proxy reports, presumably somewhat less reliable. Table 8.13 shows the proportions of individuals in each age group and each sex-and-relation-to-head group who were reported to be seriously disabled, that is, with a disability that limited the amount or type of work "a lot" (or, for children, limited activity or schooling a lot). In spite of the small numbers, the startling finding is the large proportion of other adults in families who were seriously disabled and appeared to cope with their disability by living with relatives. Even for these people, however, reports that they required extra care or costs were rare. The family heads' reports on weeks lost from work because of their own illness or that of other family members, or because of unemployment, or strike were not much affected by whether there were other disabled persons in their families. (See Appendix Tables A8.8 and A8.9.)

Racial Differences

Previous studies (Luft, 1975) have made separate estimates for blacks. Our data tend to show that racial differences are largely confined to the incidence of disability, not to its effects. Appendix Tables A8.4 through A8.7 show that

Table 8.13

PERCENTAGES WITH A SERIOUS DISABILITY,* BY AGE, SEX, AND RELATION TO HEAD

	For All Sample Individuals			
Age	Male Heads	Female Heads or Wives	Other Males	Other Females
<5			0	0
5-11			2	0
12-17		0	1	1
18-14	2	1	2	1
25-34	1	2	3	5
35-44	4	2	35**	21**
45-54	7	9	57**	11**
55-61	17	13	33**	0**
62-71	17	21	69**	7**
71-	40	30	45**	25
All Ages	9	9	2	1

*A disability reported by the family head to limit amount of type of work a lot.

**Fewer than 25 cases.

race showed no effect on the estimates of labor force participation, wage rate, or hours worked for the non-disabled, nor on the residuals of those variables for the disabled. This was true also for changes in participation, in hours worked, in deflated relative annual earnings, in transfer income, or in income/needs decile position. But blacks were 58 percent more likely to report being seriously disabled and 48 percent more likely to report some disability (Table 8.14). A comparison of Table 8.13 for all individuals with Table A8.10 for blacks only shows that blacks reported more disabilities for wives and for other family members and for individuals in almost every age group. The disabled blacks were 28 percent less likely than expected for their age, race, education, sex-marital status to work, compared with 24 percent for all the disabled--a small difference. For those who kept working, their wage rate appeared less affected in dollars, but from a lower average level, and their work hours were depressed by almost identical amounts. However, the differences in work hours according to extent of disability appeared more extreme for blacks, perhaps because of the kinds of work involved. Much more could be done to investigate these patterns, particularly with the data on change in disability and change in work.

Table 8.14

DEVIATION FROM NORMS BASED ON NON-DISABLED*
(For disabled family heads.)

How Much Disability Affects Work	Proportion Disabled		Average Residuals					
			Whether Worked		If Worked			
					Wage Rate		Hours	
	All	Black	All	Black	All	Black	All	Black
Not at all	79%	68%	--	--	--	--	--	--
Just a little	4	6	-.11	-.12	$0.92	$.28	-203	-71
Somewhat	5	6	-.12	-.19	-.60	-1.06	-389	-137
A lot	12	19	-.33	-.36	-.64	-.41	-471	-726
	100	99						
All disabled			-.24	-.28	-.71	-.30	-360	-359
Number of Cases			1,278	569	542	222	542	222
Eta^2 adjusted			.07	.05	.00	.01	.02	.12

*Norms take account (by regression) of age, race, education, sex-marital status.

The principal implication of the racial differences is clear, however. Devoting more resources to overcoming disabilities with occupational consequences is a non-discriminatory way of benefiting blacks and reducing inequalities.

SUMMARY

We found that reports of disability and changes in those reports were significantly related to labor force participation and changes in participation. Smaller effects on hours and hourly earnings also were evident, and the impact of disability on economic status was only partially offset by transfer incomes.

Estimates of the economic impact of disability were biased downward slightly if the norms were based on the whole sample rather than only the non-disabled members of the sample. The racial differences were largely in the much greater impact of disability among blacks, not on any substantially greater impact of those disabilities.

Disability was not randomly distributed, since families with disabled husbands or single heads were also more likely to include some other family members who were disabled, largely because of the substantial correlation of disability with age and age of husband with age of wife. On the other hand, reports that a disability placed a substantial burden of time or money costs on the family were quite rare.

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Appendix A

Gross and Net Explanatory Power of Various Characteristics in Accounting for Economic Status or its Change, with Special Emphasis on Disability and/or its Changes

The regressions to estimate the adjusted effects of disability or its change on work, earned income, and transfers, left little else to adjust for other than age and occasionally sex-marital status or education. The measures of explanatory power--the correlation ratio (Eta-squared) for the unadjusted effects and the analogue effects--are given in the tables which follow.

The reader should remember that seven age groups can take care of at least 98 percent of the effect of age, compared with the complete age detail, the loss from groups being very small. Since the effect is non-linear and breaks where we made our boundaries, the loss is trivial or even negative.

Table 8A.1

EXPLANATORY POWER OF DISABILITY AND OTHER THINGS WHEN USED AS CATEGORICAL PREDICTORS IN MCA-REGRESSION

Explanatory Characteristic	Work Hours*		Hourly Earnings*		Income/Needs	
	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Education	.13	.01	.13	.07	.17	.16
A & B	.39	.21	.12	.05	.05	.07
Sex-marital status	.14	.06	.08	.04	.08	.05
How much limit	.23	.05	.08	.02	.05	.02
# Other of disabled	.01	.00	.01	.00	.01	.00
R^2Adj=		.52		.26		.29

*Including 0's for non-workers (they were omitted in the regressions of Table 5).
N= 6154

Table A8.2

EFFECTS OF AGE, CHANGE IN DISABILITY (1976 TO 1978) OF VARIOUS ECONOMIC STATES-POWER OF CATEGORICAL PREDICTORS: (For family heads who worked at least 500 hours in 1972)

Explanatory Characteristic	Change in Whether Worked		Change in Work Hours		Change in Annual Earnings		Change in Transfer Income	
	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Age	.12	.08	.08	.06	.06	.04	.10	.09
Sex-marital status	.01	.00	.00	.00	.01	.01	.00	.00
Education	.02	.00	.01	.00	.02	.01	.01	.01
Change in disability	.13	.10	.04	.03	.03	.02	.06	.04
# of other disabled	.00	.00	.00	.00	.00	.00	.00	.00
R^2Adj.		.216		.098		.087		.133
Number of cases	3662		3662		3662		3662	

Table A8.3

EFFECTS OF AGE, CHANGED DISABILITY (1972-1978) ON
VARIOUS ECONOMIC STATES-POWER OF CATEGORICAL PREDICTORS
(For family heads who worked at least 500 hours in 1971 and remained heads)

Explanatory Characteristic	Change in Whether Worked		Change in Work Hours		Change in Annual Earnings		Change in Transfer Income		Change in Income/Needs Decile	
	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Age	.29	.21	.21	.17	.18	.12	.29	.28	.08	.06
Sex-marital status	.02	.00	.00	.00	.02	.01	.00	.02	.01	.00
Education	.05	.03	.04	.00	.07	.02	.02	.01	.01	.00
Change in disabled	.21	.11	.11	.05	.08	.02	.07	.03	.03	.02
E of others disabled	.01	.00	.01	.00	.01	.00	.01	.00	.00	.00
R^2Adj.		.402		.261		.288		.330		.089
N	2941		2941		2941		2941		2941	

Table A8.4

BASE REGRESSIONS USING NON-DISABLED TO ESTABLISH NORMS FOR 1977 WORK, EARNINGS

(For 4876 non-disabled family heads)

				If Worked (N=4417)			
		Whether Worked		Hourly Wage		Hours Worked	
Explanatory Characteristic	Number of Categories	Eta^2	Beta^2	Eta^2	Beta42	Eta^2	Beta^2
All	7	.43	.38	.05	.05	.10	.09
Education	10	.07	.08	.08	.08	.01	.01
Race	3	.00	.00	.01	.00	.01	.00
Sex-marital status	3	.07	.02	.05	.30	.08	.07
R^2 adjusted			.46		.17		.18
Age							
Under 34		.97	.96	$5.68	$5.64	2049	2077
35-44		.97	.96	7.87	7.56	2235	2203
45-54		.98	.97	8.74	8.60	2195	2150
55-61		.96	.96	7.61	8.14	2038	2031
62-64		.83	.85	6.33	7.34	1797	1860
65-71		.48	.50	8.27	8.76	1191	1200
72 and over		.24	.28	6.38	6.81	908	913
All		.88		$6.96		2050	

Table A8.5

REGRESSIONS ON RESIDUALS, 1977 WORK, AND EARNINGS

Explanatory Characteristic	Whether Worked		If Worked			
			Hourly Wage		Hours Worked	
	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Age	.07	.09	.03	.03	.02	.04
Education	.02	.01	.04	.04	.04	.04
Race	.00	.00	.00	.00	.00	.00
Sex-marital status	.01	.02	.00	.00	.01	.01
Disability in 1978	.07	.07	.00	.00	.02	.02
R^2 adjusted		.06		.04		.060
Mean						
Age	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.
Under 34	-.15	-.23	-$0.29	-$0.47	-399	-543
35-44	-.25	-.29	0.13	0.26	-287	-306
45-54	-.31	-.32	-2.06	-1.83	-388	-374
55-61	-.47	-.47	-0.97	-1.28	-480	-423
62-64	-.26	-.28	2.10	2.63	-386	-297
65-71	-.23	-.20	-2.59	-2.60	-240	-129
72 and over	-.13	-.09	0.55	0.04	-98	11

Note: Age and education are correlated with residuals from expected values (for non-disabled) indicating that disability has a differential effect depending on age and education!

Table A8.6

BASE REGRESSIONS USING NEVER-DISABLED TO ESTABLISH NORMS
1971 TO 1977 ANNUAL WORK AND EARNINGS
(For 2244 never-disabled family heads 1971-1978.)

Explanatory Characteristic	Number of Categories	Probability of Retiring		Change in Hours		Relative Deflated Change in Annual Earnings		Change in Transfer Income		Change in Income/Needs Decile	
		Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Age	7	.26	.25	.20	.20	.02	.02	.36	.39	.07	.07
Education	10	.02	.01	.02	.01	.00	.00	.02	.00	.00	.00
Race	3	.00	.00	.00	.00	.00	.00	.01	.00	.00	.00
Sex-marital status	3	.01	.00	.01	.01	.00	.00	.00	.00	.01	.00
OR^2 adjusted			.27		.20		.01		.38		.07
Age in 1978		Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.
Under 34		.01	.01	80	85	1.55	1.59	99	78	+.23	.21
35-44		.00	.00	-43	-51	1.58	1.58	59	25	.29	.29
45-54		.01	.01	-63	-63	1.17	1.17	111	70	.69	.68
55-61		.01	.02	-170	-174	1.03	1.01	362	392	.28	.30
62-64		.11	.11	-423	-424	.77	.68	2037	2154	-.43	-.40
65-71		.39	.39	-1364	-1372	.32	.32	5313	5428	-1.36	-1.34
72 and over		.34	.32	-833	-789	.88	.88	2903	3034	-1.24	-1.19
All		.06		-190		1.24		744		-.16	

Table A8.7

REGRESSIONS ON RESIDUALS OF CHANGE IN WORK AND EARNINGS 1971-1977

Explanatory Characteristic	Whether Retired		Change in Hours		Relative Change in Earnings Deflated		Change in Transfer Income (Undeflated)		Change in Income/Needs Decile	
	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$	Eta^2	$Beta^2$
Age	.08	.04	.04	.05	.03	.04	.07	.07	.01	.02
Education	.02	.01	.04	.03	.07	.07	.01	.02	.04	.04
Race	.00	.00	.00	.00	.00	.00	.00	.00	.01	.01
Sex-marital status	.01	.00	.00	.00	.00	.00	.01	.00	.00	.00
Change in disability	.13	.10	.09	.11	.05	.05	.02	.05	.04	.04
R^2 adjusted		.16		.147		.126		.09		.07
Age Coefficients (Not Expected Values)--Adjusted and Unadjusted										
Age	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.	Unadj.	Adj.
Under 34	-.16	-.09	-114	-279	-.20	-.36	$78	$207	-.10	-.37
35-44	-.15	-.13	114	-64	-.05	-.13	35	-23	.36	.43
45-54	-.08	-.06	7	-38	.22	.19	246	374	-.03	-.09
55-61	+.09	.07	-279	-241	-5	-.03	1438	134	-.52	-.44
62-64	-.02	-.01	-290	-227	.20	.25	382	579	.10	-.01
65-71	+.08	-.06	266	+310	.10	.18	-1034	-1127	.19	.21
72 and over	+.21	.15	192	+312	-.49	-.39	-1532	-1631	.28	.52

Table A8.8

DISABILITIES OF INDIVIDUALS BY RACE, SEX, AND WHETHER HEAD OR WIFE OR "OTHER"
(Percentages of subgroups)

	Husbands, Wives, and Single Heads				Other Individuals in Families			
	Male		Female*		Male**		Female**	
Race	Some Disability	Affects A Lot	Some Disability	Affects A Lot	Some Disability	Affects A Lot	Some Disability	Affects A Lot
White	17	9	16	8	4	2	3	1
Black	29	18	28	15	5	4	5	3
Other	16	7	12	6	4	1	5	2
All	18	10	17	9	4	2	4	1
N	3,371		4,473		4,000		3,711	

*About half these are reported for by husbands.
**Mostly reported by family head, not by the individual.

Table A8.9

DISABILITIES OF INDIVIDUALS BY AGE AND RACE
(For all 15,555 sample persons)
(Percentages of subgroups.)

	White		Black		All*	
Age	Some Disability	Affects Work or School A Lot	Some Disability	Affects Work or School A Lot	Some Disability	Affects Work or School A Lot
Under 12	3	1	2	1	3	1
12-17	3	1	4	1	3	1
18-24	4	1	7	4	5	2
25-34	5	1	11	4	6	2
35-44	8	3	12	6	8	4
45-54	15	8	32	16	16	9
55-61	23	12	46	36	25	15
62-64	25	13	43	21	28	14
65-71	34	21	63	44	37	23
72 and over	49	33	79	55	52	35
All ages	12	6	17	10	12	6
N		8,592		6,338		15,555

*Includes 625 individuals neither black nor white (mostly Hispanic).

Table A8.10

DISABILITIES OF INDIVIDUALS BY AGE, SEX, AND WHETHER HEAD OR WIFE
(Percentages of subgroups.)

	Husbands, Wives, and Single Heads				Other Individuals in Families			
	Male		Female		Male		Female	
Age of Individual	Some Disability	Limits A Lot	Some Disability	Limits A Lot	Some Disability	Limits A Lot	Some Disability	Limits A Lot
Under 12	--	--	--	--	3	1	2	0
12-17	--	--	*	*	3	1	3	1
18-24	7	2	5	1	4	2	3	1
25-34	8	1	4	2	5	3	5	5
35-44	9	4	6	3	*	*	*	*
45-54	15	8	16	8	*	*	*	*
55-61	27	17	24	13	*	*	*	*
62-64	29	7	27	17	*	*	*	*
65-71	37	23	39	23	*	*	*	*
72-	56	41	50	31	*	*	*	*
N		3,371		4,473		4,000		3,711

*Fewer than 25 cases.

Table A8.11

DISABILITIES OF BLACKS BY AGE, SEX, AND WHETHER HEAD OR WIFE OR "OTHER"

(Percentages of black subgroups.)

	Husbands, Wives, and Single Heads				Other Individuals in Families			
	Male		Female		Male		Female	
Age	Some Disability	Affects A Lot	Some Disability	Affects A Lot	Some Disability	Affects A Lot	Some Disability	Affects A Lot
Under 12	--	--	--	--	3	2	1	0
12-17	--	--	*	*	3	0	6	2
18-24	9	5	7	2	9	8	4	1
25-34	14	5	9	3	5	4	6	3
35-44	11	3	11	5	*	*	*	*
45-54	25	11	27	19	*	*	*	*
55-61	41	39	50	34	*	*	*	*
62-64	47	11	39	27	*	*	*	*
65-71	62	51	64	38	*	*	*	*
72-	90	66	88	54	*	*	39	37
N	997		1,634		1,893		1,814	

*Too few cases.

Chapter 9

REVERSE ANNUITY MORTGAGES: A DISSAVING MECHANISM FOR OLDER HOMEOWNERS

Thomas Neubig

INTRODUCTION

Current money income is an incomplete measure of the economic status of families. This is especially true for elderly families whose current money income is low because they are retired, yet who have accumulated assets over several decades of work. The largest of these assets is usually the single family home. Imputed rental income from owner occupied homes would raise a significant fraction of poor elderly homeowners above the official poverty line, but few of them can take advantage of their relatively illiquid net housing equity without selling their homes. Imputed rental income is of little consolation to elderly homeowners who may need to dissave their housing equity but wish to remain in their homes. Many older families could be aided by a financial instrument that would enable them to utilize the equity they have saved as homeowners.

With the introduction of a variety of home equity dissaving instruments, many elderly homeowners may be able to take advantage of their home equity within the next few years and still remain in their homes. As of January 1, 1979, the Federal Home Loan Bank Board authorized federal savings and loan associations to offer home equity dissaving plans which have been reviewed by the Bank Board (Federal Register, 1978). A number of states have taken steps to authorize similar programs for lenders under their jurisdiction. This paper describes two of a variety of possible plans that fall under the rubric of Reverse Annuity Mortgages (RAMs). Both of these plans would provide inflation-adjusted payments for the life of the homeowner and spouse while they remain in their home. The payments would be dispersed gradually from a fixed-balance loan with the home as collateral. The loan would be repaid out of the estate or when the home is sold.

In this paper, we first review the price appreciation of houses between 1972 and 1977. The future price appreciation of houses can provide older homeowners with a valuable cushion against inflation if incorporated in RAMs. Next, we describe two RAM plans which incorporate inflation-adjustment features in the dissaving process. Finally, we discuss the long-term effects of the availability of these RAMS, focusing on their impact on long-term poverty and the possibility of elderly homeowners remaining in their homes longer. Payment amounts from these two "optimal" dissaving plans provide useful upper-limit estimates of payment amounts for other future home equity dissaving plans.

PRICE APPRECIATION OF HOMES, 1972-1977

The price of new houses skyrocketed during the 1970s. The price of an average new house rose from $32,700 in 1972 to $54,200 in 1977, an increase of 66 percent.[1] The values of existing homes also increased greatly. The average owner-occupied home increased in value by 62 percent, from $22,400 in 1972 to $36,400 in 1977.[2] However, prices of homes occupied by the elderly are often thought to have appreciated less than other homes since the elderly are apt to live in older structures, often in need of upkeep and in economically declining neighborhoods. While these factors are certainly important in some cases, data indicate that, in general, the prices of homes of the elderly have mostly outpaced the rate of inflation.

We examined the price appreciation of homes by comparing reported house values in 1972 and 1977 for different age groups. While reported house values may be criticized as estimates of "actual" home values, values reported at two points in time by the same head of a family which remained in the same home over that period probably provide a better measure of the percentage change in the house value or rate of price appreciation than values of different homes at two points in time. In addition, an elderly homeowner's *perceptions* of the house value and its rate of appreciation would undoubtedly be of major importance in a decision to dissave through a RAM.

Our sample included 1,632 homeowners from the Panel Study of Income Dynamics who remained in the same homes between 1972 and 1977. Families which changed their abodes during the period were excluded.

Two measures of price appreciation which we used are presented in Table 9.1. The first compared the average house value in 1972 to the average house value in

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[1] Construction Reports C27, Price Index of New One-Family Houses Sold, U.S. Department of Commerce, Bureau of the Census.

[2] Sample estimates of reported house values from the Panel Study of Income Dynamics.

1977; the increase of 57 percent was substantially greater than the 45 percent rise in the consumer price index over the same five-year period. The percentage increase declined with the age of the family head, ranging from 62 percent for family heads under 45 years of age to 40 percent for family heads older than 75. By this measure, only the homes of the oldest family heads appreciated at a rate below the rate of inflation, and just barely so. In terms of annual compound rates of appreciation, the homes of family heads who were 75 or older appreciated at a rate of 7.0 percent, compared to a 7.7 percent annual rate of inflation.

A second measure of the appreciation of homes takes into account each individual homeowner's experience by comparing the value of each house in 1972 to its value in 1977. The average ratio of individual house values between 1972 and 1977 was 1.60. Results for this second measure ranged from 1.69 for the youngest family heads to 1.49 for the oldest. The homes of the elderly appreciated at a significantly lower rate than did the homes of persons younger than 65, but they did appreciate at a greater rate than inflation (although not significantly so).

Change in average house values was also calculated for all homeowners in both 1972 and 1977, including families that moved to different homes. The results were generally the same for the different age groups. The only significant difference occurred among family heads younger than 55, whose house values tended to be larger in 1977 by a greater percentage than the restricted sample of non-movers. This was due to young families upgrading their housing stock by moving into larger and possibly newer homes.

The difference in appreciation rates between age groups can be partially explained by changes in the housing stock due to more home improvements or less depreciation rather than changes in house values due to changes in demand or inflation. A cross-tabulation of homeowners in 1971 showed that non-elderly homeowners were 50 percent more likely than elderly homeowners to make additions or repairs on their homes. This biased the estimated rates of appreciation upwards for the homes of non-elderly families, possibly accounting for the difference.

The distribution of individual rates of appreciation of home values revealed that elderly homeowners were twice as likely to report no change or an actual decline in their house value compared with non-elderly homeowners (Table 9.2). Nevertheless, only about 15 percent of the elderly homeowners reported no appreciation of their homes between 1972 and 1977. Some of them may simply have been uninformed about current market prices of homes. The vast majority of the elderly homeowners, however, did perceive the increasing value of their homes.

Table 9.1

PRICE APPRECIATION OF HOMES--1972 to 1977--
BY AGE OF HOUSEHOLD HEAD
(For all families remaining in the same homes between 1972 and 1977.)

Age of family head in 1977	Average Ratio of 1977 House Value to 1972 House Value	Average 1972 House Value	Average 1977 House Value	Ratio of Average 1977 House Value to Average 1972 House Value	Unweighted Number of Observations	Percent of Weighted Observations
Younger than 35	1.69	$19,520	$31,320	1.60	138	6.9%
35-44	1.66	25,270	40,890	1.62	297	16.1
45-54	1.64	25,500	40,420	1.59	477	26.3
55-64	1.61	23,140	35,920	1.55	367	22.4
65-74	1.52	20,000	31,590	1.58	233	17.9
75 or older	1.49	16,620	23,350	1.40	120	10.4
All families	1.60	22,620	35,510	1.57	1632	100.0

Table 9.2

DISTRIBUTION OF PRICE APPRECIATION OF HOMES—1972 to 1977—
BY AGE OF FAMILY HEAD
(For all families living in the same homes in 1972 and 1977.)

Age of Family Head in 1977	Average Annual Rate of Price Appreciation--1972-1977								Number of Unweighted Observa-tions	Percent of Weighted Distributions
	Less than 0%	0%	Less than 5%	5.0-9.9%	10.0-14.9%	15.0-19.9%	20% or More	Total		
Younger than 35	6%	2%	11%	31%	30%	10%	10	100%	138	6.9%
35-44	4	2	18	34	23	9	10	100	297	16.1
45-54	2	3	17	35	27	9	7	100	477	26.3
55-64	4	2	18	34	25	10	7	100	367	22.4
65-74	11	3	19	32	20	9	6	100	233	17.9
75 or older	9	7	24	32	12	7	9	100	120	10.4
All families	6	3	18	33	23	9	8	100	1632	100.0

Homeowners' perceptions of their homes' rate of appreciation is important to the potential demand for RAMs, as well as for other home equity dissaving plans. Plans requiring a homeowner to forego the right to the future appreciation of his or her house without some adjustment for future inflation (such as a sale-leaseback plan) are clearly undesirable. The experience of elderly homeowners during the five-year period considered is consistent with the expectation that, in the absence of some unusually large decrease in demand or increase in supply, real assets, such as houses, appreciate at close to the rate of inflation.

CURRENTLY AVAILABLE HOME EQUITY DISSAVING PLANS

Currently, a homeowner has few alternatives that could cash out the value of his or her house without selling it. The most common means of dissaving the equity in the home is to let it depreciate through lack of maintenance. In response to a recent Louis Harris poll which asked elderly respondents what they would do with an extra $100, the most frequent answer--given by 21 percent of the respondents--was to make needed additions or repairs to their homes (Harris, 1979). Deterioration of a home is a destructive means of dissaving because both the value of the house and the quality of the housing are reduced.

A conventional loan would temporarily increase an elderly homeowner's funds, but the principal would have to be repaid during the life of the homeowner. Moving to a smaller home would free some of the original home's equity but would leave the smaller home's value illiquid and would involve both monetary and psychic costs. Selling the home and moving into an apartment would free the entire home equity but would leave the elderly person with the possibility of outliving his or her assets. A sale-leaseback arrangement would not require leaving the house, but any future capital gains on it would be lost and there would still be the possibility of outliving the value of the homeowner's remaining assets unless he or she bought an annuity with the funds.

Home equity dissaving mechanisms can take a variety of forms from property tax postponement plans to sale-leaseback arrangements to reverse annuity mortgages. All have the common feature of providing additional liquidity to homeowners without having them move from their homes. Other aspects of the plans differ greatly. An optimal dissaving plan would protect the elderly from three major economic risks: (1) mortality risk—the possibility of outliving one's assets; (2) inflation--the possibility of substantial reduction of real purchasing power; and (3) depreciation of one's capital--the possibility of the house falling into disrepair or the neighborhood deteriorating. The mortality risk could be covered by the purchase of a lifetime annuity, that would provide regular payments for the life of the purchaser and the spouse. The right to the future capital gain on the home could provide some protection against inflation. An automatic provision indexing the annuity payments to the Consumer Price Index would eliminate transaction costs, uncertainty, and time lags involved with refinancing a loan each time the house value increased. Part of the third risk, the depreciation of the house, could be reduced by an insurance provision providing major maintenance and repairs to homes participating in the plan.

REVERSE ANNUITY MORTGAGES

Two plans called Reverse Annuity Mortgages could protect elderly homeowners against each of the three types of risk. These plans would involve only one transaction on the part of the homeowner--from a loan to be repaid out of the homeowner's estate, the homeowner would receive monthly payments for life. From the issuing institution's perspective, a RAM would involve two steps: (1) the negotiation of a non-amortized loan with a simultaneous purchase of a lifetime annuity, and (2) regular payments to the homeowner after subtracting the interest charge. The loan would be fixed in value, since the principal would be repaid only when the homeowner dies or the house is sold, yet the interest would be paid each term out of the annuity payment.

Table 9.3 compares a RAM with the alternative choices presently open to homeowners wishing to cash out some of the equity in their homes. A RAM would not only enable the homeowner to retain possession and use of the home while dissaving the equity, it would also allow the homeowner to choose the rate of dissaving, to capture any price appreciation of the home, and to leave his heirs the option to purchase the family home out of the estate.

The introduction of a variety of home equity dissaving plans increases the choices available to elderly homeowners. They need no longer be forced to choose between selling their homes or remaining there with past savings tied up in the home equity. Among all the possible home equity dissaving instruments, RAMs would best protect the homeowner against some major economic risks facing the elderly today.

The amount of the monthly or annual payments from a Reverse Annuity Mortgage would depend on four factors: (1) the net equity (the house value minus any remaining mortgage debt) of the homeowner; (2) the percentage of the net equity the homeowner can borrow; (3) the interest rate offered by the financial institution; and (4) the life expectancy of the homeowner and possible survivor.

Table 9.3

CHOICES AVAILABLE TO ELDERLY HOMEOWNERS WISHING TO CASH OUT THEIR HOUSE'S EQUITY

Choices	Cash Out the Net Equity	Life Tenure in the House	Capture the Price Appreciation	Protection from Out-living One's Capital	Option for Heirs Keeping the Family Home
Living in the home and leaving a large estate	No	Yes	Yes	Yes	Yes
Selling the home and moving to a smaller house	Partially	No	Yes	Yes	No
Moving to a rental residence	Yes	No	No	No	No
Sale-and-lease	Yes	No	No	No	No
Conventional loan	No	Maybe	Yes	No	Maybe
Reverse annuity mortgage	Yes	Yes	Yes	Yes	Yes

A loan-to-net-equity ratio of 80 percent is assumed in the remaining sections. Leaving a fraction of the net equity for the homeowner would provide an incentive to maintain the home and keep up its value. The interest rate used on the loan would also be used on the annuity by a single institution. An interest differential, with the loan interest rate higher than the annuity interest rate, would reduce the payment levels considerably.

The shorter the life expectancy of the homeowner, the larger the annuity payments would be. Table 9.4 shows the possible increase in the payment levels for delayed purchase of a RAM plan and the annual payment levels per $10,000 of net equity at various ages. It is assumed that married couples would take out a joint and two-thirds survivor annuity. The surviving spouse would receive two-thirds of the original payment when the first spouse dies. The calculation of the annuity payment is described in Appendix A.

The first of the two Reverse Annuity Mortgage plans--which we will call RAM1 in the discussion below--would take the form of a regular non-amortized loan combined with the purchase of a lifetime annuity. The interest rate used would be the current market interest rate for conventional mortgage loans. The loan would be open-ended so that it could be renegotiated if the value of the house increases. Refinancing of the loan would provide a larger supplement to the original payment due to the shortened remaining life expectancy of the homeowner.

A second and similar plan--RAM2--would trade part of the potential future appreciation of the house in return for a lower interest rate and inflation-indexed annuity payments. The issuing institution would offer a three percent rate of interest on its capital while realizing the inflation premium from any future appreciation of the home. The payments would be automatically indexed for inflation, thereby providing the homeowner some protection against it.

The Initial Impact of a Reverse Annuity Mortgage Plan

This section describes the potential effects that a RAM2 plan would have had, if such a plan had been initiated in 1977, on the economic status of elderly families. RAM payments were calculated for 729 elderly homeowning families using their 1977 net housing equity, marital status, and age of the family head and spouse. The impact of the RAM was measured in absolute terms; that is, we calculated the actual amount of an average annual RAM payment, the percentage increase that a RAM payment would have had on a family's total income, the proportion of elderly families that would have been raised above the official poverty line, and the reduction in the poverty gap.

Table 9.4

ANNUAL RAM PAYMENTS PER $10,000 NET EQUITY FOR DIFFERENT AGES

Age		Approximate Life Expectancy[a]	Annual Payment Per $10,00 Net Equity[b]		Percentage Increase in Payment If Wait Beyond Age 65	
Single Female	Single male		RAM2 (3%)	RAM1 (9%)	RAM2 (3%)	RAM1 (9%)
55	50	27.0	$ 192	$ 69	-	-
60	55	21.7	262	119	-	-
65	60	18.2	330	174	-	-
70	65	15.0	422	252	28%	45%
75	70	12.1	548	365	66	110
80	75	9.6	719	525	118	202
85	80	7.5	951	747	188	330

[a] Internal Revenue Code § 1.72-9.

[b] Eighty percent of the net equity is used in the calculation.

The average annual payment from a RAM2 calculated in 1977 with a 3 percent interest rate for all elderly homeowners would have been $1,330, with higher benefits later from the inflation adjustment (Table 9.5). The average payment would have been larger for homeowners age 75 or older due to their shorter life expectancy. RAM payments would have been less for elderly married couples since on average they were several years younger than the single elderly and since their expected joint lives and survivor life would have approximately equalled the life expectancy of a younger single female. Families below the poverty line in 1977 would have received annual RAM payments averaging $650, less than half the average payments to non-poor families. The difference was due almost entirely to the lower net equity in homes that poor families had ($11,600 in 1977 compared with $27,900 for non-poor families; Table 9.5).

Table 9.5

HOUSING NET EQUITY, ANNUAL RAM PAYMENTS AND
RAM PAYMENTS AS A PROPORTION OF 1977 INCOME

		RAM2 (3%)		RAM1 (9%)	
Age or Poverty Status	Average Housing Net Equity, 1977	Average Annual Payment	Proportion of 1977 Income	Average Annual Payment	Proportion of 1977 Income
Age of Family Head					
65-74	$29,020	$1,190	0.140	$710	0.084
75 or older	21,830	1,550	0.242	1,130	0.177
Poverty Status, 1977					
Poor	11,630	650	0.306	440	0.212
Non-poor	27,910	1,400	0.165	910	0.109
All Elderly Homeowners	26,340	1,330	0.178	860	0.119

Over 70 percent of all elderly families were homeowners in 1977. If the RAM2 plan had had universal coverage or had covered even a substantial fraction of elderly homeowners, it could have boosted the average income of elderly families greatly. Thirty percent of all elderly families in 1977 would have received monthly RAM2 payments of $100 or more. Another 25 percent would have received monthly payments of over $50 but less than $100. Sixteen percent would have received less than $50 a month from RAMs initially (Table 9.6). These calculations assume the participation of all elderly home-owning families from the inception of this hypothetical program in 1977. Some elderly families would probably have waited until they were older or until they needed financial assistance from the plan in order to increase their future payment level.

Table 9.6

MONTHLY RAM2 PAYMENTS
(Percentage distribution of elderly families.)

	Monthly RAM2 Payments (3%)									
Age of Family Head	None	Less than $50	$50-99	$100-149	$150-199	$200-249	$250 or More	Total	Number of Unweighted Cases	Percent of Weighted Cases
65-74	28%	18%	28%	15%	8%	1%	2%	100%	495	61.8%
75 or older	31	14	19	15	9	6	6	100	234	38.2
All Elderly Families	29	16	25	15	9	2	4	100	729	100.0

A RAM2 plan would have provided elderly homeowners with initial annual payments equalling almost 18 percent of their 1977 total family income. The proportion of income represented by RAM plans would have been 24 percent for family heads over age 75, compared to 14 percent for those aged 65 to 74. This difference is due to the larger RAM payments for older family heads as well as the higher incomes of some of the the younger heads who were still in the labor force. Despite the relatively small payments to elderly poor homeowners, RAM2 payments would have constituted an additional 30 percent of their income in 1977. That fraction increases in importance when it is recalled that these payments would have been fixed in real terms and would not decline in purchasing power with inflation.

The annual payments and proportion of income represented by RAM1 (that is, at market interest rates, about 9 percent in 1977) would have been considerably smaller than those for RAM2 as shown in Table 9.5. The difference is greatest for heads close to age 65 since the impact of compound interest is larger the

Table 9.7

POTENTIAL INCREASE IN ANNUAL INCOME REPRESENTED BY ANNUAL RAM2 PAYMENT

Age of Family Head	Annual RAM2 Payment (3%)/ 1977 Total Family Income								Number of Unweighted Cases	Percent of Weighted Cases
	None	Less than 0.10	0.10-0.19	0.20-0.39	0.30-0.39	0.40-0.49	0.50 or More	Total		
65-74	28%	33%	25%	9%	2%	2%	1%	100%	495	61.8%
75 or older	31	16	24	11	6	5	7	100	234	38.2
All Elderly Families	29	26	25	10	4	3	3	100	729	100.0

longer the period of the loan, or in the case of the RAM the longer the life expectancy.[3]

Eleven percent of elderly families had incomes below the official poverty line in 1977. Household heads age 75 or older were more likely to be poor in a single year than elderly heads age 65 to 74 (25.6 percent compared to 8.8 percent). RAM2 would have reduced the poverty population of the elderly in 1977 from 11.4 percent to 7.2 percent. Overall, one-third of the elderly poor could have been raised out of poverty by initial participation in a RAM. Sixty percent of the elderly homeowners whose money incomes were below the poverty line would have been raised above it by the addition of RAM payments. The impact of the RAM1 would have had nearly the same effect, reducing the proportion of elderly poor to 7.8 percent (Table 9.8).

The relationship between future RAM payments and government transfer programs remains to be determined. RAM payments would not affect Social Security payments since they would be reduced only by labor income. Whether Supplemental Security Income payments and Food Stamp benefits would be reduced by

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[3] A higher interest rate, even with no interest rate differential between the annuity and loan rate, reduces the payment amount, since the interest on the loan is paid on the entire loan principal while the interest on the annuity is earned on a declining balance.

Table 9.8

POVERTY STATUS OF ELDERLY FAMILIES WITH AND WITHOUT REVERSE ANNUITY MORTGAGES

Age of Family Head	Percent of Families Below the Poverty Line, 1977			Number of Unweighted Cases	Percent of Weighted Cases
	Without RAMs	With RAM2 (3%)	With RAM1 (9%)		
65-74	8.8%	5.1%	5.7%	495	61.8%
75 or older	15.6	10.6	11.0	234	38.2
All Elderly Families	11.4	7.2	7.8	729	100.0

participation in a RAM will be important in determining whether poor elderly homeowners would participate in RAM plans (Chen, 1977).[4]

Tables 9.9 and 9.10 show the cross-tabulation of the actual 1977 ratios of income to needs with the post-RAM2 ratios. The needs level has been adjusted to represent the poverty line, so any family with a pre-RAM ratio of less than one fell below the official poverty line. The bottom two rows show the cumulative percentage of families below certain income/needs ratios both with and without a RAM2. Without a RAM, 15.7 percent of elderly families with heads older than 75 were poor in 1977, while with a RAM2 only 10.6 percent would have been poor. Twenty percent of all elderly families were below 125 percent of the poverty line in 1977 and 30 percent fell below 150 percent of the poverty line. With RAM2s, 15 and 24 percent, respectively, would have fallen below these measures of near poverty.

The "poverty gap" is the amount of income needed by poor families to raise them up to the poverty line. The average poverty gap for the poor elderly homeowners was $645 in 1977. The supplemental cash provided by RAMs would have reduced the average poverty gap for these families by two-thirds, to $210.

The potential initial impact of a Reverse Annuity Mortgage plan is considerable even according to the above estimates which were calculated for all elderly homeowners without allowing for possible postponement of participation

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[4] Nine percent of all elderly families would have been poor in 1977 after receipt of RAM2 payments if Supplemental Security Income was reduced dollar for dollar after the $20 monthly disregard.

Table 9.9

PRE- AND POST- RAM2 INCOME/NEEDS RATIO—1977
(For family heads age 65 or older.)

Post-RAM2 Income/ Needs Ratio, 1977	Pre-RAM Income/Needs Ratio, 1977						
	Less than 1.0	1.00- 1.24	1.25- 1.49	1.50- 1.99	2.00- 2.99	3.00- 4.99	5.00 or Above
Less than 1.00	7.2%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
1.00-1.24	2.6	5.1	0.0	0.0	0.0	0.0	0.0
1.25-1.49	1.5	2.2	5.4	0.0	0.0	0.0	0.0
1.50-1.99	0.2	1.0	3.7	8.8	0.0	0.0	0.0
2.00-2.99	0.0	0.2	1.1	7.3	15.9	0.0	0.0
3.00-4.99	0.0	0.0	0.0	0.0	6.1	17.9	0.0
5.00 or more	0.0	0.0	0.0	0.0	0.0	3.0	11.0
Total pre-RAM	11.4	8.5	10.2	16.0	22.0	20.9	11.0
Total post-RAM2 (3%)	7.2	7.6	9.1	13.6	24.5	24.0	14.0
Pre-RAM cumulative percent	15.0	19.9	30.1	46.1	68.1	89.0	100.0
Post-RAM2 (3%) cumulative percent	7.2	14.8	23.9	37.5	62.0	86.0	100.0
N = 729							

until beyond age 65 in order to increase their benefits or until they were in need of the benefits. The next section looks at the potential impact of a RAM plan over a longer time horizon than just one year. The effect of an inflation-indexed benefit and a refinancing mechanism is discussed. We then turn to an estimate of the long-term results of what is perhaps the largest benefit from RAMs—their potential to help elderly families remain in their homes by increasing the housing choices available to them.

The Long-Term Impact of Reverse Annuity Mortgages

We have shown that Reverse Annuity Mortgages would have a substantial impact on poverty within a one-year period. The potential impact of RAMs over a longer time horizon also has promise in the alleviation of long-run poverty and in helping elderly homeowners remain in their homes. Long-run poverty among older families is a quite serious problem. A family hit with emergency medical expenses or the loss of a spouse's pension might never regain its former economic

Table 9.10

PRE- AND POST- RAM2 INCOME NEEDS RATIO--1977
(For family heads age 75 or older.)

	Pre-RAM Income/Needs Ratio, 1977						
Post-RAM2 Income/ Needs Ratio, 1977	Less than 1.0	1.00-1.24	1.25-1.49	1.50-1.99	2.00-2.99	3.00-4.99	5.00 or Above
Less than 1.00	10.7%	0.0%	0.0%	0.0%	0.0%	0.0%	0.0%
1.00-1.24	3.3	5.7	0.0	0.0	0.0	0.0	0.0
1.25-1.49	1.3	2.5	7.0	0.0	0.0	0.0	0.0
1.50-1.99	0.4	1.6	4.2	6.8	0.0	0.0	0.0
2.00-2.99	0.0	0.5	2.9	7.5	14.4	0.0	0.0
3.00-4.99	0.0	0.0	0.0	0.0	7.1	12.5	0.0
5.00 or more	0.0	0.0	0.0	0.0	0.0	3.4	8.5
Total pre-RAM	15.7	10.2	14.0	14.3	21.5	15.8	8.5
Total post-RAM2 (3%)	10.6	8.9	10.9	13.0	25.2	19.5	11.9
Pre-RAM cumulative percent	15.7	25.9	39.9	54.2	75.7	81.5	100.0
Post-RAM2 (3%) cumulative percent	10.6	19.5	30.4	43.4	68.6	88.1	100.0

N = 234

position. Nearly twice as many elderly families were poor at some time from 1972 to 1977 as were poor in 1977, and this relationship held true for both homeowners and renters. Twenty-three percent of all elderly families were poor in at least one of the years during the six-year period, compared to 11 percent who were poor in a single year and 12 percent who were poor in at least three years out of the six (Table 9.11).

This section examines the impact that an inflation-adjusted RAM2 initiated in 1972 at a 3 percent interest rate would have had on the 415 elderly homeowners who remained in the same homes between 1972 and 1977. Eighteen percent of these homeowning families were poor at least once during the six year period. Nine percent were poor in at least half of the six years. The RAM2 plan would have provided payments raising 30 percent of the poor homeowners above the poverty line for all six years. If all of the elderly homeowners could have started to participate in a RAM program in 1972, only five percent would have been poor for as many as three of the six years (Table 9.12).

Table 9.11

HOUSING STATUS AND POVERTY STATUS, 1972 AND 1977[a]

Housing Status	Percent Poor 1972	Percent Poor 1977	Percent Poor at least... Years Between 1972 and 1977: One	Percent Poor at least... Years Between 1972 and 1977: Three	Unweighted Number of Observations	Percent of Weighted Observations
Owner 1972-1977 same home	7.1%	9.2%	18.5%	9.4%	415	59.4%
Owner 1972-1977 different home	8.7	9.7	21.6	9.0	63	9.3
Owner 1972, Renter or other arrangement, 1977	18.3	14.7	40.2	23.7	47	7.3
Renter 1972-1977 same structure	7.9	7.4	22.4	9.7	90	11.9
Renter 1972-1977 different structure	12.3	19.7	32.0	13.3	62	6.5
Other living arrangements 1972-1977	18.0	32.6	52.1	34.2	52	5.6
All Families	9.1	11.4	23.7	12.0	729	100.0

[a] This sample includes all families with heads 65 or older in 1977 and compares their 1977 housing status with their 1972 housing status. Change in family composition is not taken into account.

Table 9.12

LONG-RUN POVERTY STATUS OF ELDERLY HOME-OWNING FAMILIES 1972-1977, WITH AND WITHOUT RAMS

(For elderly home-owning families.)

		With RAM2 (3%)		With RAM1 (7.6%)	
Age or Marital Status	Without RAMs	Unadjusted	Inflation-Indexed	Unadjusted	Refinanced
Percent Poor At Least One Year Between 1972 and 1977					
Age of Head in 1977					
65-74	17.1%	12.7%	12.1%	14.7%	14.4%
75 or older	21.1	14.6	13.9	15.8	15.1
Marital Status					
Married	12.4	8.1	7.4	10.1	10.1
Single male	20.1	17.6	17.6	17.6	17.6
Single female	25.2	18.5	17.8	20.3	19.2
All families	18.5	13.4	12.8	15.1	14.7
Percent Poor At Least 3 Years Between 1972 and 1977					
Age of Head in 1977					
65-74	8.9%	4.9%	4.9%	7.0%	5.9%
75 or older	10.3	6.7	5.8	7.4	7.4
Marital Status					
Married	5.0	2.3	2.3	3.4	3.0
Single male	10.8	5.9	5.9	10.8	8.3
Single female	14.1	9.2	8.4	10.5	9.7
All families	9.4	5.5	5.1	7.1	6.3

N = 415

The inflation-adjustment increases the favorable effect only slightly more than an unadjusted RAM2. Since the poverty line is adjusted for increases in consumer prices, the favorable benefits from an inflation-adjusted RAM did not show up clearly in this measure. Inflation-adjusted payments would not have been likely to raise additional poor families above the poverty line, but they would have kept families' income above the inflation-adjusted poverty line. The benefits of the inflation adjustment, however, would have increased over time. A longer time period than six years would show a larger difference between the impact of the unadjusted and inflation-adjusted RAM2.

The RAM1 plan, using market interest rates (7.6 percent in 1972), was calculated for the same homeowners. A refinancing provision was included which calculated a supplemental RAM1 payment when the house value increased by at least $5,000. The supplemental payment was based on the current market interest rate, the shortened life expectancy of the homeowner and the additional value of the house. The unadjusted RAM1 would have reduced the proportion of poor elderly homeowners in long-run poverty by half the reduction of the RAM2 plan, similar to the 1977 cross-sectional results. The refinancing provision would have reduced the proportion of families in long-run poverty by 0.8 percent over and above the 2.3 percent reduction that would have resulted from the basic benefit (Table 9.12). As with the RAM2 plan, the effect of RAM1's refinancing provision would have been larger for the years furthest from the initial year. A longer time period and a less conservative refinancing provision would have increased the effect of the adjustments for inflation.

RAMs would quite probably enable many elderly homeowners to remain in their homes for a longer time. The true potential in this area can not be determined in this paper, but we can provide estimates of the number of people who might be helped in this way. Seventy-six percent of the family heads older than 65 in 1977 owned homes in 1972. Five years later 60 percent still owned the same homes, 9 percent owned different homes and 7 percent had moved from their own homes and become renters or made some other arrangement. One in eight, or roughly 1,850,000, elderly families moved out of their own homes during the five year period. The Panel Study does not provide enough information to assess the reasons for the moves. Retirement to another geographical area, the death of a spouse, or medical problems are all reasons for a change of residence which would not be affected by dissaving devices. Other reasons include large medical expenses or rising property taxes, maintenance costs, and utility bills. Unforeseen expenses or higher homeownership costs are cases in which RAMs would provide a new alternative to the present decision of whether or not to sell the home. Unmarried and older household heads were most likely to have moved from their own home (Table 9.13). These same individuals and families would have received the largest RAM payments relative to their present income.

The importance of RAM payments relative to total family income would increase with time. Since inflation-adjusted payments grow relative to the portion of income that is fixed in nominal terms, the ratio of inflation-adjusted RAM payments to total income would become greater over time.

SUMMARY

This chapter has examined the perceived change in house values among homeowners between 1972 and 1977. Increased house values were reported less often by older homeowners than by younger homeowners. Family heads who were 65 or older reported a 9.0 percent average annual price appreciation, while those who were younger than 65 reported an average rate of 10.2 percent. All age groups, however, experienced average increases in house values larger than the increases in consumer prices between 1972 and 1977.

Home equity dissaving instruments would provide important supplementary cash to homeowners. Upper-limit estimates from two potential plans show that unlocking the net equity in a home would reduce both short-run and long-run poverty among older families.

Two Reverse Annuity Mortgage plans were described that would not only make housing equity available to homeowners during their lifetimes, but would also protect them against the risks of outliving their assets, against inflation, and against possible depreciation of their houses. Choices about which type of RAM plan and when to begin participation could significantly affect the benefits received from using the home equity that is for many families their most important financial asset.

Table 9.13

HOUSING STATUS—1972 AND 1977[a]

Age or Marital Status	Owner 1972-1977		Owner 1972, Renter or Other Arrangement, 1977	Renter, 1972-1977		Other Living Arrangements 1972-1977	Total	Unweighted Number of Observations	Percent of Weighted Observations
	Same Home	Different Home		Same Structure	Different Structure				
Age of Family Head, 1977									
65-74	60%	10%	6%	13%	6%	5%	100%	495	61.8%
75 or older	58	9	10	10	7	6	100	234	38.2
Marital Status, 1977									
Married	69	12	3	9	3	4	100	299	43.2
Single male	49	10	13	12	8	8	100	85	11.7
Single female	53	7	10	14	10	6	100	345	45.1
All Families	59	9	7	12	7	6	100	729	100.0

[a] This sample includes all families with heads 65 or older in 1977 and compares their 1977 housing status with their 1972 housing status. Change in family composition is not taken into consideration.

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Appendix 9.1

The annual RAM payment received by a homeowner with a fixed debt non-amortized loan is calculated by the formula in Equation 1.

$$1)\quad A_n = \alpha V \frac{r}{e^{rn} - 1} = \alpha V \cdot \frac{r}{1 - e^{-rn}} - \alpha V r$$

where n is the life expectancy in years, r is the interest rate, α is the ratio of loan to net equity, V is the present value of the house net equity, and e is Euler's constant for continuous compounding. The calculation is simply the portion of the net equity that is to be annuitized times the equivalent annual stream of income from one dollar for a given interest rate and number of years minus the annual interest charge on the borrowed funds.

The life expectancies used for the calculation were from the Internal Revenue Code Regulation § 1.72-9. The life expectancies were slightly longer than those reported in the Statistical Bulletin (May 1977) from the National Center for Health Statistics. All heads' ages were converted into the equivalent single female's age. Five years were added to the age of single male heads. Married couples have approximately the same life expectancy for a joint and two-thirds survivorship as a single female. The joint age used for the life expectancy table converted the male's age into the equivalent female's age. A uniform age for both spouses was found by adding a fraction of the difference between the ages found in a Table of Uniform Seniority (I.R.C. Reg. § 1.72), to the younger age. Subtraction of two years from the uniform age approximated the equivalent single female age for a joint and two-thirds survivorship life expectancy.

Chapter 10

A COMPARISON OF UTILITY PAYMENTS AND BURDENS BETWEEN 1971 AND 1977*

Richard D. Coe

In 1971 the fuel and utilities component of the Consumer Price Index stood at 115.1, up from 100.0 in 1967. This equalled an annual increase for the four-year period of 3.6 percent. By 1977, the CPI for fuel and utilities was 202.2, representing an annual increase for the six-year period of 9.8 percent. Large price increases such as these can be expected to impose financial burdens on many, if not most, households and should invoke behavioral changes as consumers attempt to avoid these burdens.

The policy debate over how to deal with growing shortages of energy resources continues. One major area of contention is whether the price of energy resources should be allowed to rise further and, if so, by how much. In answering this question it would be useful for policymakers to know what the distributional burden of higher utility costs has been and what options are open to consumers in responding to higher prices. This analysis is directed toward providing some answers to these questions.

The Organization of the Analysis

The analysis is divided into two major sections, with each section containing two subsections. The first part of the chapter is a descriptive look at the distribution of utility payments and burdens from 1971 and 1977. It begins by looking at the distribution by income and income/needs deciles for all households in the Panel Study for those two years. Because the data on utility payments for homeowners is considerably more reliable than that for renters (since many renters have their utility bills, or a portion thereof, included in their rent) we also look at the distribution for homeowners only. The second part of the first section involves a more detailed look at the distribution of burdens for homeowners. Annual utility payments and burdens for 1977 and 1971 are shown by various demographic characteristics, and the change in payments and burdens over the six-year period are discussed.

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The second section of the chapter involves an econometric examination of the determinants of the change in the annual utility expenditures of homeowners. The first part of this section deals with the estimation of a general expenditure function, while the second part estimates the more conventional linear expenditure function. The results from the different specifications are compared. The last section of the chapter summarizes the major findings of the analysis, and briefly discusses their policy implications.

I. THE AGGREGATE DISTRIBUTION OF UTILITY PAYMENTS AND BURDENS

All Households

In 1971 the average annual utility payment for the 3,796 households was $331, representing 4.3 percent of family income (Tables 10.1 and 10.2). The payments ranged from an average of $187 for households in the lowest income decile to $480 for households in the highest decile. When households are ranked according to a ratio of their income to the cost of their minimum basic needs, the range is lessened. Households in the lowest income/needs decile averaged $228 in payments, while households in the highest decile averaged $409 in utility payments. Utility bills in 1971 clearly burdened lower income households more than higher income households, as the percentage of income devoted to utility bills averaged 12.3 percent for households in the lowest income decile compared to 1.8 percent for households in the highest income decile. Even these figures understate the actual disparity in burden between low and high income households, primarily because a substantially larger percentage of households in the lowest income decile reported no utility payments at all (20.6 and 3.0 respectively). (Table 10.3). Presumably, lower income households are more likely to have their utilities included in their rent. Of the households in the lowest income decile which reported some utility payments, 64 percent spent at least one-tenth of their income for utilities. In the highest income decile, no household spent more than 7.5 percent of its income on utilities.

For the 5,787 households in the sample in 1977, annual utility payments averaged $685, a 107 percent increase from the annual average utility payment in 1971. (This increase occurred despite the fact that the percentage of all households which reported no utility payments increased from 8.2 to 11.8 percent between 1971 and 1977). Households in the lowest income decile averaged $345 in annual utility payments, while households in the highest income decile averaged $1,083. As in 1971, this range decreased when households were ranked according to their income/needs ratio.

Table 10.1

MEAN UTILITY PAYMENTS BY INCOME AND INCOME/NEEDS
DECILES, 1971 AND 1977 (For all households.)

	1977		1971	
Decile	Income	Income/Needs	Income	Income/Needs
Lowest	$345	$423	$187	$228
Second	440	502	252	257
Third	520	632	263	300
Fourth	569	563	283	316
Fifth	649	710	310	341
Sixth	709	686	326	344
Seventh	734	803	361	347
Eighth	829	760	362	359
Ninth	912	815	403	364
Highest	1,083	932	480	409
All	$685	$685	$331	$331
Number of observations	5,787		3,796	

The percentage of income which went to the payment of utility bills increased from 4.3 percent in 1971 to 5.9 percent in 1977, a 37 percent increase in the burden of utility payments. As in 1971, households in the lower income deciles were substantially more burdened than those with higher incomes. On average, households in the lowest income decile spent 15.0 percent of their income on utilities, while households in the highest income decile spent only 2.5 percent of their income on utilities. Again, we find that even these figures understate the disparity in the burden between low and high income households, as Table 10.4 indicates. In 1977 fully one-third of the households in the lowest income decile reported no utility payments, while only 2.4 percent of the households in the highest income decile reported paying no utility bills.

When the figures for 1971 and 1977 presented in Tables 10.1 and 10.2 are compared across income deciles, it appears that households in the higher income deciles were somewhat more adversely affected by rising utility prices. For example, the percentage increase in average annual utility payments for

Table 10.2

MEAN UTILITY BURDEN BY INCOME AND INCOME/NEEDS DECILES, 1971 AND 1977 (For all households.)

	1977		1971	
Decile	Income	Income/Needs	Income	Income/Needs
Lowest	15.0	15.0	12.3	11.4
Second	9.1	9.0	7.5	6.8
Third	7.1	8.0	5.3	5.7
Fourth	5.8	5.3	4.2	4.5
Fifth	5.2	5.4	3.8	3.7
Sixth	4.6	4.5	3.2	3.4
Seventh	4.0	4.2	3.0	2.9
Eighth	3.7	3.4	2.5	2.5
Ninth	3.2	3.1	2.2	2.2
Highest	2.5	2.4	1.8	1.7
All	5.9	5.9	4.3	4.3
Number of observations	5,787		3,796	

households in the lowest income decile was 84 percent, while the comparable increase for households in the highest income decile was 126 percent. Similarly, the average burden for households in the lowest income decile increased 22 percent from 1971 to 1977, while for households in the highest income decile the increase was 39 percent.

One might be led to conclude from this that rising utility prices were more burdensome for higher income households. However, a comparison of Tables 10.3 and 10.4 yields a different, and probably more accurate, conclusion. The main reason for the lower increases for households in the lowest income decile was the fact, noted above, that the percentage of these households which reported no utility payments increased from 20.6 percent to 33.5 percent. We found above that 64 percent of those households in the lowest income decile which reported some utility payments in 1971 spent more than 10 percent of their income on utilities. By 1977 this figure had increased to 77 percent. Perhaps more significantly, in 1977 31.1 percent of all households in the lowest income decile

Table 10.3

DISTRIBUTION OF UTILITY BURDEN BY INCOME DECILES, 1971
(1971 utility payments as percent of income, for all households.)

1971 Income Decile	No Utility Payments	0.1-0.9%	1.0-2.4	2.5-4.9	5.0-7.4	7.5-9.9	10.0-14.9	15.0-19.9	20.0% or More	Total
Lowest	20.6	0.0	0.3	6.4	11.5	10.2	21.5	14.7	14.8	100.0
Second	13.0	0.4	1.8	16.8	21.1	20.7	19.1	5.8	1.3	100.0
Third	14.5	0.9	2.7	33.5	25.3	11.7	8.7	2.5	0.1	100.0
Fourth	11.3	0.1	21.4	33.3	22.3	7.0	3.5	1.1	0.0	100.0
Fifth	8.5	2.0	13.8	53.3	16.7	3.9	1.8	0.0	0.0	100.0
Sixth	6.6	2.2	26.7	51.3	10.6	1.9	0.8	0.0	0.0	100.0
Seventh	4.8	2.3	29.4	55.2	7.1	0.7	0.4	0.0	0.0	100.0
Eighth	2.6	3.7	48.9	41.6	3.2	0.0	0.0	0.0	0.0	100.0
Ninth	3.1	8.9	54.0	31.4	2.6	0.0	0.0	0.0	0.0	100.0
Highest	3.0	16.2	58.9	21.5	0.3	0.0	0.0	0.0	0.0	100.0
All	8.2	4.0	27.7	35.5	11.5	5.0	4.8	2.0	1.3	100.0
N=	3,796									

Table 10.4

DISTRIBUTION OF UTILITY BURDEN BY INCOME DECILES, 1977
(N=5,787) (1977 utility payments as percentage of income, for all households.)

1977 Income Decile	No Utility Payments	0.1-0.9%	1.0-2.4	2.5-4.9	5.0-7.4	7.5-9.9	10.0-14.9	15.0-19.9	20.0% or More	Total
Lowest	33.5	0.3	0.5	4.7	5.4	4.7	10.6	9.3	31.1	100.0
Second	21.9	0.7	2.3	9.0	11.8	11.1	24.2	11.7	7.3	100.0
Third	18.7	0.0	7.4	14.9	15.8	16.4	17.9	6.6	2.4	100.0
Fourth	15.7	1.3	9.9	17.3	27.4	13.4	10.9	2.5	1.5	100.0
Fifth	7.7	2.3	12.8	30.1	24.3	15.1	6.3	0.6	0.8	100.0
Sixth	6.2	4.0	13.3	35.7	26.3	9.6	4.6	0.0	0.2	100.0
Seventh	6.5	4.1	13.0	48.4	21.5	5.3	1.1	0.0	0.0	100.0
Eighth	4.6	1.9	20.9	49.6	18.1	4.5	0.3	0.0	0.0	100.0
Ninth	3.5	2.5	27.7	55.9	8.3	2.1	0.0	0.0	0.0	100.0
Highest	2.4	8.4	43.1	40.8	4.6	0.4	0.2	0.0	0.0	100.0
All	11.8	2.6	15.4	31.1	16.3	8.1	7.4	3.0	4.2	100.0

were spending more than 20 percent of their income on utilities, more than double the percentage in 1971. In contrast, we found in 1971 that no household in the higher income deciles spent more than 7.5 percent of its income on utilities. In 1977, 0.6 percent were spending this much, a trivial increase. Although it is difficult in the absence of data on the relationship of rents and utilities to say what the overall effect of rising utility prices has been on low-income households, it seems clear that a sizeable fraction of low-income households have been greatly burdened by higher utility payments. Households in the higher income deciles apparently have been able to increase their incomes sufficiently to offset the severity of rising utility payments.

Homeowners Only

Because of the problems involved with determining the actual cost of utilities for renters, the above analysis was repeated for only those families who owned their homes. In 1971 there were 2,030 homeowners in the sample; in 1977 there were 2,980. The average annual utility payments for homeowners was $397 in 1971, 20 percent higher than for all households. The average ranged from $257 for homeowners in the lowest income decile to $514 for homeowners in the highest income decile (Table 10.5). Utility payments on average took up 4.8 percent of homeowners' incomes. The distribution of this burden was steeply regressive, as the percentage of income devoted to utility payments averaged 17.5 percent for homeowners in the lowest income decile compared to only 1.9 percent for homeowners in the highest income decile (Table 10.6). The severity of this burden on low-income homeowners is further emphasized in Table 10.7. Almost one-quarter (23.5 percent) of homeowners in the lowest income decile spent over 20 percent of their income for utilities in 1971. No homeowners in the highest three income deciles spent more than 7.5 percent of their income for utilities.

In 1977, the average annual utility payments for homeowners was $858, a 116 percent increase over the six-year period. Homeowners in the lowest income decile averaged $593, a 131 percent increase from 1971, while homeowners in the highest income decile averaged $1,134, a 121 percent increase. The percentage of income devoted to utilities averaged 6.7 percent in 1977, a 40 percent increase from 1971. This increased burden fell most heavily upon low-income homeowners. Homeowners in the lowest income decile paid on average over one-quarter (26.0 percent) of their income for utilities in 1977, almost a 50 percent increase from 1971. Homeowners in the highest income decile paid only 2.6 percent of their income for utilities, a 37 percent increase from 1971. The percentage of

Table 10.5

MEAN UTILITY PAYMENTS BY INCOME AND INCOME/NEEDS DECILES 1971 AND 1977
(For all homeowners.)

	1977		1971	
Decile	Income	Income/Needs	Income	Income/Needs
Lowest	$593	$694	$257	$280
Second	643	676	323	335
Third	707	799	340	370
Fourth	754	755	371	380
Fifth	807	840	361	392
Sixth	807	812	391	400
Seventh	828	965	405	413
Eighth	900	874	391	410
Ninth	972	912	445	425
Highest	1,134	1,024	514	468
All	$858	$858	$397	$397
N		2,980		2,030

homeowners in the lowest income decile who spent more than 20 percent of their income on utilities more than doubled from 1971 to 1977, from 23.5 percent to 58.3 percent (Tables 10.7 and 10.8). Virtually none of the homeowners in the highest three income deciles spent more than 10 percent of their income for utilities in 1977.

A More Detailed Look at the Utility Payments and Burdens of Homeowners

A Cross-Sectional Look. Table 10.9 shows the distribution of utility payments and burdens in 1977 and 1971 by a selected set of characteristics of the household. The sample is limited to homeowners in both years who reported some utility payments in each of the two years, a total of 1,823 households. The average annual utility payments for these households more than doubled in the six-year period, from an average of $400 in 1971 to an average of $864 in 1977. The percentage of income which went for utility bills increased from 4.6 percent to 6.9 percent, a 50 percent increase.

Table 10.6

MEAN UTILITY BURDEN BY INCOME AND INCOME/NEEDS DECILES, 1971 AND 1977
(For all homeowners.)

	1977		1971	
Decile	Income	Income/Needs	Income	Income/Needs
Lowest	26.0	25.5	17.5	15.9
Second	13.4	12.5	9.7	9.2
Third	9.6	10.0	6.9	6.9
Fourth	7.7	7.0	5.5	5.4
Fifth	6.5	6.2	4.4	4.0
Sixth	5.3	5.1	3.8	3.9
Seventh	4.5	4.8	3.3	3.4
Eighth	4.0	3.8	2.7	2.8
Ninth	3.4	3.3	2.5	2.5
Highest	2.6	2.6	1.9	1.8
All	6.7	6.7	4.8	4.8
N	2,980		2,030	

In both 1977 and 1971 utility expenditures increased as the price of utilities, household income, house value, the number of rooms in the structure, home food expenditures, and the number of persons in the household increased. Despite this fact, however, the burden of utility payments decreased as these factors increased. Households headed by self-employed persons reported higher utility payments than those headed by non-self-employed persons, perhaps indicating that self-employed persons may include some business utility expense in their reported utility payments.

Perhaps the most interesting results relate to four commonly used descriptive variables--region, race, age, and poverty status. In both 1977 and 1971, families residing in the Northeast had the highest utility expenditures. Households in the South had the lowest average annual payments in 1971, but by 1977 households in the West had by far the lowest average payments. In fact, while the burden of utility payments increased sharply between 1971 and 1977 for households in the rest of the country, the average burden experienced by households residing in the West actually decreased slightly.

Table 10.7

DISTRIBUTION OF UTILITY BURDEN BY INCOME DECILES, 1971
(N=2,030)
(1971 utility payments as percent of income, for all homeowners.)

1971 Income Decile	No Utility Payments	0.1–0.9%	1.0–2.4	2.5–4.9	5.0–7.4	7.5–9.9	10.0–14.9	15.0–19.9	20.0% or More	Total
Lowest	1.4	0.0	0.0	3.4	10.4	10.4	30.8	20.1	23.5	100.0
Second	0.6	0.0	0.3	7.8	25.7	27.1	27.6	9.3	1.7	100.0
Third	2.0	0.0	0.4	33.1	29.7	18.3	11.5	5.0	0.0	100.0
Fourth	0.7	0.0	12.9	34.0	33.5	12.2	4.7	2.0	0.0	100.0
Fifth	0.1	0.7	8.5	60.1	23.5	5.7	1.4	0.0	0.0	100.0
Sixth	0.5	0.0	19.9	60.7	14.6	3.0	1.2	0.0	0.0	100.0
Seventh	0.3	0.8	24.5	64.4	8.5	1.0	0.6	0.0	0.0	100.0
Eight	1.0	1.2	46.3	48.1	3.3	0.0	0.0	0.0	0.0	100.0
Ninth	0.0	4.8	54.9	37.0	3.3	0.0	0.0	0.0	0.0	100.0
Highest	0.4	12.9	62.7	23.6	0.4	0.0	0.0	0.0	0.0	100.0
All	0.6	2.9	28.9	39.5	12.9	5.8	5.4	2.4	1.6	100.0

Table 10.8

DISTRIBUTION OF UTILITY BURDEN BY INCOME DECILES, 1977
(N=2,980) (1977 utility payments as percent of income, for all homeowners.)

1977 Income Decile	No Utility Payments	0.1- 0.9%	1.0- 2.4	2.5- 4.9	5.0- 7.4	7.5- 9.9	10.0- 14.9	15.0- 19.9	20.0% or More	Total
Lowest	1.8	0.0	0.0	3.9	3.2	3.6	14.5	14.7	58.3	100.0
Second	2.1	0.0	0.0	7.0	8.4	13.1	36.7	21.1	11.5	100.0
Third	2.9	0.0	0.7	12.7	20.9	23.0	28.1	8.3	3.4	100.0
Fourth	2.7	0.0	4.6	15.7	34.7	20.2	16.4	3.7	1.8	100.0
Fifth	1.7	0.5	3.7	32.2	30.1	20.0	10.1	1.0	0.7	100.0
Sixth	0.0	.09	8.0	42.9	32.5	11.4	4.0	0.0	0.3	100.0
Seventh	1.6	1.0	9.9	54.8	24.8	6.5	1.4	0.0	0.0	100.0
Eighth	0.9	0.8	17.9	55.1	20.4	4.4	0.4	0.0	0.0	100.0
Ninth	1.1	0.6	26.4	59.9	9.9	2.1	0.0	0.0	0.0	100.0
Highest	0.4	6.5	43.3	44.2	5.1	0.5	0.0	0.0	0.0	100.0
All	1.3	1.4	15.2	38.2	18.5	9.0	8.3	3.3	4.6	100.0

Table 10.9 (page 1 of 4)

AVERAGE ANNUAL UTILITY PAYMENTS AND BURDEN BY SELECTED DEMOGRAPHIC CHARACTERISTICS, 1971 AND 1977 (For homeowners in both years who reported some utility payments in each year.)

Demographic Characteristics	1977 Number of Observations	1977 Weighted Percent	1977 Average Annual Utility Payments	1977 Average Utility Burden	1971 Number of Observations	1971 Weighted Percent	1971 Average Annual Utility Payments	1971 Average Utility Burden
All	1,823	100.0	$864	.069	1,823	100.0	$400	.046
Utility Price Index								
1971								
Less than 65					432	23.6	376	.048
65-69					253	14.2	378	.047
70-74					486	20.6	359	.051
75-99					314	19.7	433	.049
100 or more					338	21.9	452	.038
1977								
Less than 100	123	7.5	787	.084				
100-119	445	23.1	758	.066				
120-139	8[illegible]	43.9	846	.071				
140-199	241	13.0	877	.068				
200 or more	164	12.5	1,158	.057				
Income								
Less than $5,000	211	11.2	621	.203	299	15.8	305	.117
$5,000-9,999	293	16.2	679	.093	511	24.3	356	.049
$10,000-14,999	291	14.6	804	.065	472	25.9	405	.033
$15,000-19,999	276	14.7	829	.048	270	16.1	432	.025
$20,000-29,999	402	21.8	926	.038	208	13.6	471	.020
$30,000-49,999	293	17.9	1,095	.030	53	3.6	588	.016
$50,000 or more	57	3.7	1,303	.020	10	0.7	845	.013

Table 10.9 (page 2 of 4)

Demographic Characteristics	1977				1971			
	Number of Observations	Weighted Percent	Average Annual Utility Payments	Average Utility Burden	Number of Observations	Weighted Percent	Average Annual Utility Payments	Average Utility Burden
Heating Degree Days								
Less than 1,500 degrees	322	11.8	802	.084	374	13.4	350	.049
1,500–1,999	375	18.0	743	.069	350	17.8	356	.053
2,000–2,999	294	16.2	926	.070	264	14.6	380	.040
3,000–3,299	239	15.6	853	.064	348	22.3	430	.045
3,300–3,999	397	26.9	948	.067	290	20.2	452	.044
4,000 degrees or more	196	11.5	849	.063	197	11.7	405	.049
Cooling Degree Days								
Less than 300 degrees	113	7.1	763	.057	233	14.0	400	.046
300–399	190	12.0	902	.059	90	6.6	491	.043
400–499	219	15.0	1,018	.070	368	24.1	451	.048
500–649	404	25.3	824	.060	280	17.5	389	.044
650–999	388	19.7	877	.072	382	19.4	352	.046
1,000 or more	509	20.9	804	.085	470	18.4	362	.049

Table 10.9 (page 3 of 4)

	1977				1971			
Demographic Characteristics	Number of Observations	Weighted Percent	Average Annual Utility Payments	Average Utility Burden	Number of Observations	Weighted Percent	Average Annual Utility Payments	Average Utility Burden
Number of Rooms								
Three or less	49	2.8	447	.112	70	3.8	331	.089
Four	239	13.0	722	.086	243	12.5	340	.064
Five	425	23.2	749	.081	485	26.3	349	.046
Six	547	28.6	864	.066	521	27.8	391	.041
Seven	290	16.7	954	.052	261	14.9	449	.038
Eight or more	273	15.7	1,133	.051	243	14.6	530	.040
House Value								
Less than $20,000	538	23.8	718	.109	1,116	54.9	352	.055
$20,000-34,999	542	29.1	806	.070	513	32.0	422	.036
$35,000-49,999	386	24.4	875	.053	136	9.0	514	.037
$50,000-74,999	241	15.4	1,003	.043	48	3.5	615	.033
$75,000 or more	116	7.3	1,244	.040	10	0.7	665	.035
Type of Structure								
Single family	1,639	89.6	874	.067	1,519	88.6	402	.045
Apartment, other	102	5.9	884	.090	143	7.1	395	.046
Trailer	82	4.5	650	.077	89	4.3	374	.069
Family Size								
One	258	17.1	639	.131	158	10.1	322	.102
Two	543	34.3	791	.064	463	30.3	364	.049
Three	300	15.7	892	.048	285	15.8	390	.036
Four	294	15.4	969	.049	328	18.1	418	.032
Five or more	428	17.5	1,112	.052	589	25.6	467	.038
Home Food Expenditures								
Less than $1,000	194	11.7	612	.131	317	18.4	319	.076
$1,000-1,499	239	13.8	683	.091	455	24.7	364	.045
$1,500-1,999	319	17.9	783	.068	431	23.8	389	.043
$2,000-2,499	309	16.7	891	.057	279	15.2	442	.033
$2,500-2,999	270	14.2	855	.047	187	9.7	475	.032
$3,000 or more	492	25.8	1,119	.048	154	8.2	563	.037

Table 10.9 (page 4 of 4)

Demographic Characteristics	1977 Number of Observations	1977 Weighted Percent	1977 Average Annual Utility Payments	1977 Average Utility Burden	1971 Number of Observations	1971 Weighted Percent	1971 Average Annual Utility Payments	1971 Average Utility Burden
Housework Hours								
Less than 750	281	15.4	729	.082	284	15.0	385	.053
750-1,499	561	31.0	854	.077	567	31.7	384	.047
1,500-1,999	363	19.9	890	.065	381	21.1	403	.047
2,000-2,499	276	14.8	942	.058	289	15.6	407	.044
2,500-2,999	164	9.1	947	.060	133	7.3	430	.042
3,000 or more	178	9.8	860	.054	169	9.3	441	.039
Employment Status								
Not self-employed	1,618	88.5	837	.070	1,612	88.2	393	.046
Self-employed	205	11.5	1,071	.062	211	11.8	453	.047
Race								
White, other	1,455	93.2	857	.065	1,455	93.2	405	.045
Black	368	6.8	958	.114	368	6.8	339	.064
Age of Head								
Less than 65	1,389	70.4	928	.054	1,529	81.2	413	.040
65 or more	434	29.6	712	.104	254	18.8	346	.076
Poverty Status								
Not poor	1,702	95.7	874	.060	1,670	94.4	407	.040
Poor	121	4.3	643	.268	153	5.6	290	.155
Region								
Northeast	312	22.3	1,072	.066	313	22.3	489	.044
North Central	535	32.5	863	.067	543	33.0	406	.046
South	701	29.0	830	.087	694	28.6	338	.050
West	275	16.2	644	.043	273	16.1	376	.044

The situation of black homeowners is particularly interesting. In 1971, blacks had lower average annual utility payments than nonblack homeowners--$339 vs. $405 respectively. By 1977, however, their average annual utility payments were considerably higher than that for nonblacks--$958 to $857. This resulted in a dramatic increase in the burden of utility payments on black homeowners--from 6.4 percent in 1971 to 11.4 percent of income in 1977, a 5 percentage point increase, as compared to a 2 percentage point increase for nonblacks.

The burden of utility expenditures also was high for elderly homeowners and homeowners who were in poverty, but for reasons quite different than those for black homeowners. In both 1971 and 1977, the poor and the elderly spent considerably less on utilities than did the nonpoor and nonelderly. Despite this they still had to devote a substantially greater proportion of their income for utility bills. The poor in particular were hard hit. In 1977, poor homeowners spent on average over one-quarter of their cash income--26.8 percent--for utilities.

These groups--the poor, the elderly, and blacks--no doubt overlap each other to a degree. It is also possible that they reside disproportionately in areas of more severe weather. In order to acquire some idea of the burden borne by each of these groups, independent of the weather and to control for the overlap between groups, a simple regression was run with utility burden (multiplied by 100) as the dependent variable and as independent variables heating and cooling degree days (a measure of weather conditions), whether black, whether elderly, and whether poor. The results are presented in Table 10.10. It is evident that the weather is not the reason why blacks, the elderly, and the poor suffer from heavy utility burdens. Controlling for differences in heating and cooling degree days does not alter the fact that these groups are forced to devote a substantially higher percent of their income for the payment of utility bills.

A Longitudinal Look. Cross-sectional comparisons such as those presented above disguise the possibility that individual households may change their position between the two years. An alternative way of describing the effect of rising utility costs is to relate the change in utility payments and burdens between 1971 and 1977 to the change in variables of interest experienced by these households over the six-year period. This is done in Table 10.11. The results, in general, are very similar to what was found in the cross-sectional comparisons. Larger increases in income and house value were associated with larger increases in utility expenditures but smaller increases in the burden of such expenditures. Becoming self-employed resulted in substantially larger

Table 10.10

DEMOGRAPHIC REGRESSIONS FOR UTILITY BURDEN, 1971 AND 1977,
(For homeowners in both years who reported some utility payments in each year.)
(N=1,823)

Independent Variable	1977	1971	Change in Burden, 1971-1977
Constant	-.158	3.27	.96
Heating Degree Days (10 degrees) (or change in)	.012**	.002	.025**
Cooling Degree Days (10 degrees) (or change in)	.023**	-.004	.062**
Whether Black	2.51**	.34	1.75**
Whether Elderly	3.52**	2.55**	1.46**
Whether Poor	19.08**	10.98**	-
(Always Non-Poor)	-	-	-
Became Non-Poor	-	-	-8.16**
Became Poor	-	-	21.23**
Always Poor	-	-	4.91**
R^2	.371	.287	.247

Significance levels: **.01

increases in utility expenditures, while those who remained self-employed also had larger than average increases. Households which fell into poverty had below average increases in utility expenditures, but because of their falling income had considerably higher than average increases in the burden of such expenditures--an average increase which amounted to almost one-quarter of their cash income. Again we see that while households residing in the rest of the country in 1977 on average had increases in the burden of utility payments, households residing in the West actually experienced on average a slight decrease in their utility burden.

A MULTIVARIATE ANALYSIS OF THE CHANGE IN UTILITY EXPENDITURES OF HOMEOWNERS

Thus far we have seen that the level of utility payments and burdens increased substantially from 1971 to 1977, and that the expense of utilities weighs considerably more heavily on lower income households than on higher income households. Looking at homeowners only, we have also seen how the level of expenditures and burdens, and changes in such, are related to selected economic and demographic characteristics and changes in them. In this section our goal is to pinpoint via a multivariate econometric analysis the determinants of change in the level of utility payments. We limit our sample, as in the previous section, to homeowners in both 1971 and 1977 who reported some utility payments in each of the two years, a total of 1,823 households.

A major issue in expenditure analysis is the proper specification of the equation to be estimated. In this section, alternative specifications are examined and the results compared. In the first part of this section no specific form of the expenditure function is assumed, and an approximation of the total differential of the function is estimated. In the second part of the section the more conventional linear expenditure function is assumed, and the first difference of the equation is estimated, as well as the one-year cross-sectional specification. Double logarithmic forms of the cross-sectional equations are also estimated and the results given in the Appendix.

The analysis in this section is clearly in the spirit of what Brown and Deaton appropriately label the "practical" school of expenditure estimation.[1] No attempt is made to determine whether the specifications postulated here

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[1] Brown and Deaton (1972)

Table 10.11 (page 1 of 3)

MEAN CHANGE IN UTILITY PAYMENTS AND BURDENS, 1971-1977
(For homeowners in both years who reported
some utility payments in each year.)

Household Characteristic	Number of Observations	Weighted Percent of Sample	Mean Change in Utility Payments	Mean Change in Utility Burden
All	1,823	100.0	$464	2.23
Change in Utility Price Index				
Less than 40 points	295	17.6	306	1.03
40-44	181	7.9	542	4.43
45-49	528	23.7	473	2.88
50-59	295	18.7	427	1.81
60-74	304	16.7	473	2.11
75 or more	220	15.3	624	2.14
Change in Money Income				
Less than $0	311	17.7	384	8.16
+0 - +4,999	562	31.4	381	2.21
+5,000 - +9,999	404	20.9	470	0.77
+10,000 - +19,999	391	20.8	570	0.10
+$20,000 or more	155	9.2	649	-0.91
Change in Heating Degree Days				
Less than -100 degrees	430	26.5	$320	0.89
-100 - -1 degrees	194	12.0	483	2.36
0 - +74 degrees	161	7.1	637	2.48
+75 - +149 degrees	326	18.4	589	3.11
+150 degrees or more	712	36.0	465	2.68
Change in Cooling Degree Days				
Less than -100 degrees	23	1.4	385	0.10
-100 - -1 degrees	418	26.7	405	0.91
0 - +74 degrees	608	36.9	496	2.41
+75 - +149 degrees	291	14.4	485	2.45
+150 degrees or more	483	20.7	473	3.60

Table 10.11 (page 2 of 3)

Household Characteristic	Number of Observations	Weighted Percent of Sample	Mean Change in Utility Payments	Mean Change in Utility Burden
Change in Number of Rooms				
Less than zero	353	19.1	420	1.89
No change	982	55.9	464	2.61
+1	293	15.1	462	1.54
+2 or more	145	9.9	549	1.84
Change in House Value				
Less than zero	161	7.4	338	2.60
+$0-7,499	483	23.6	427	3.37
+$7,500-14,999	473	27.2	433	2.23
+$15,000-24,999	358	21.1	461	2.10
+$25,000 or more	348	20.6	596	0.94
Change in Type of Structure				
Remained in single family house	1,531	85.4	467	2.17
Apartment to house	73	2.9	657	5.41
Trailer to house	35	1.5	433	1.43
House to apartment	32	1.8	347	2.69
Remained in apartment	69	4.1	545	4.44
Trailer to apartment	1	0.1	- 66	0.38
House to trailer	28	1.6	279	-0.97
Apartment to trailer	1	0.1	210	2.32
Remained in trailer	53	2.8	282	-0.32
Change in Family Size				
-2 or less	341	16.3	466	2.36
-1	413	22.8	426	3.09
Zero	832	50.1	456	1.79
+1 or more	237	10.8	579	2.30
Change in Real Home Food Expenditures				
Less than -$1,000	256	13.7	472	1.85
-$1,000 - -501	364	19.9	430	2.52
-$500 - -$1	598	35.7	416	2.59
-$0 - +$499	394	21.0	499	2.14
+$500 or more	211	9.7	624	1.19

Table 10.11 (page 3 of 3)

Household Characteristic	Number of Observations	Weighted Percent of Sample	Mean Change in Utility Payments	Mean Change in Utility Burden
Change in Housework Hours				
Less than -1000	230	12.2	440	2.30
-1000 - -501	259	14.0	485	3.46
-500 -1	344	18.9	409	1.46
0 - +499	476	27.5	509	2.57
+500 - +999	253	14.2	442	1.71
+1000 or more	261	13.2	473	1.84
Change in Self-Employment Status				
Always not self-employed	1,524	83.4	444	2.22
Became self-employed	88	4.7	663	1.94
Became not self-employed	94	5.1	496	1.85
Always self-employed	117	6.7	546	2.84
Race				
White, other	1,455	93.2	453	2.03
Black	368	6.8	619	4.97
Age of Head, 1977				
Head under age 65	1,350	68.2	514	1.60
Head age 65 or more	473	31.8	356	3.60
Change in Poverty Status				
Always nonpoor	1,610	92.3	470	1.87
Became nonpoor	92	3.4	390	-5.45
Became poor	60	2.1	391	24.01
Always poor	61	2.2	392	8.77
Region in 1977				
Northeast	312	22.3	582	2.19
North Central	535	32.5	459	2.12
South	701	29.0	491	3.73
West	275	16.2	262	-0.14

conform to the restrictions of conventional demand theory--namely, symmetry, negativity, aggregation, and homogeniety.

A General Expenditure Function

The Model. We hypothesize that total expenditures on utilities of the ith household is a function of the price of utilities faced by the household, its income, and a set of demographic characteristics of the household, about which more will be said below. Thus we have:

(1) $E_i = f(P_i, Y_i, D_i)$

Totally differentiating (1) yields

(2) $dE_i = \frac{\partial f}{\partial p} dP_i + \frac{\partial f}{\partial Y} dY_i + \frac{\partial f}{\partial D} dD_i$

Equation 2 simply states that the change in utility expenditures of the ith household is the sum of the responsiveness of expenditures to changes in the different variables times those actual change in those variables.[2] The responsiveness of the household to changes in certain parameters is given by the partial differential of the expenditure function with respect to that parameter. Elasticities are defined as $\frac{\partial E}{\partial x} \cdot \frac{x}{E}$; consequently, the least squares coefficients obtained from Equation 2 will allow an estimate of the elasticity of expenditures with respect to variables of interest.[3]

In addition to changing incomes and prices, other factors can be expected to influence the change in total utility expenditures. The weather, of course, is one. Temperature fluctuations should lead to increased use of airconditioning and/or furnaces, thus leading to changing utility bills. Changes in the characteristics of the dwelling unit in which the family resides, such as moving to a larger home, would also be expected to alter utility payments. Changing characteristics of the family itself, such as the number of persons in the household, should also affect utility payments.

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[2] This specification ignores any interrelationship between the arguments of the expenditure function. For example, one of the demographic variables to be included in the equation is house value, which is most likely a function of income. Thus, a change in income, while exerting a direct effect on change in expenditures, would be expected also to exert an indirect effect via its influence on changing house values. We assume that these indirect influences are small and can be ignored.

[3] It should be noted that the derivation of elasticities in the form specified in the text assumes small changes in the value of the variable of interest. In this analysis the changes are measured over a six-year period, 1971-1977, and consequently are large. For example, the mean change in the CPI for utilities experienced by the sample was 58 percentage points. Thus it would perhaps be more appropriate to replace the differential symbols in Equation 2 with delta symbols to represent larger changes.

To measure the effect of these influences, the following variables, in addition to change in income and price,[4] were entered into the regression equation, with change in annual utility payments as the dependent variable. Some of the variables have either been mentioned above or are self-explanatory and thus are not discussed. All changes refer to the change from 1971 to 1977.

1. Change in Weather Conditions[5]
    a. Change in annual heating degree days: A measure of the change in heating requirements faced by the household. Data are based on the state of residence of the household in the respective year. Heating degree days are measured as the difference in the mean temperature for a given day from 65°F, if the mean temperature is below 65° F. Illustratively, if the mean temperature for a day were 30°F, the heating degree days for that day would equal 35.
    b. Change in annual cooling degree days: A similar measure as above, but designed to measure the change in cooling requirements faced by the household.
2. Change in the characteristics of the Dwelling Unit
    a. Change in number of rooms in the dwelling unit.
    b. Changes in house value: Although purely inflationary increases in the value of the residential structure would not be expected to influence utility expenditures, changes in house value which reflect improvements to or deterioration of the structure may affect utility payments.
    c. Change in type of structure: The type of structure in which a family resides has been grouped in three categories: single-family structure, trailer, apartment or other. An apartment unit includes duplexes. Changes in the type of structure in which the family resides (and owns) can be expected to change utility expenditures, although the expected direction of the change is not clear a priori.
3. Changes in Household Utilization Characteristics
    a. Change in family size.
    b. Change in real home food expenditures: The more intensely a household utilizes its dwelling unit, the higher should be its utility payments. One possible measure of the change in the intensity of the use of the home is the change

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[4] The price data is based on individual consumer price indices for fuel and utilities for the 22 largest SMSA's in the coterminous United States. My thanks to Robin Barnes of the Department of Labor for providing this data.

[5] Data taken from U.S. Environmental Data Service, 1971 and 1977.

in home food expenditures (in real terms). Cooking more meals at home can be expected to lead to higher utility bills. This measure is imprecise because there is no way of ascertaining whether a change in real food expenditures represents a change in the utilization of the dwelling unit or a change in the quality of food consumed.

c. Change in total housework hours: Another measure of the intensity of use of the dwelling unit. An increase in the amount of work done around the house can be expected to result in increased utility expenditures.

d. Change in self-employment status: Some self-employed persons may work out of their homes and might not separate business and personal utility expenses. Consequently a change in self-employment status would be expected to change annual utility payments.

There is another set of characteristics of the household whose causal relationship to change in utility payments is somewhat tenuous but which is included in the estimation equation because of the interest for policy purposes and for their possible explanatory power, even if the reason for any such power is a matter of speculation. These characteristics are whether the head of the household is age 65 or older, whether the head is black, and the change in the poverty status of the household. The equation is estimated both with and without these variables included. Finally, a constant term not present in Equation 2 has also been included in order to account for any general increase in utility expenditures (due, for example, to rising living standards) which may have occurred from 1971 to 1977.

The regression results are presented in Table 10.12. A comparison of Columns 1 and 2 shows that the inclusion of race, age, and poverty status did not substantively alter the estimated coefficients on the other variables, consequently the discussion below is based on the results shown in Column 2.

Most independent variables were highly significant in the expected direction. The strict economic variables, price and income, were both positively correlated with utility expenditures. With every percentage point increase in the Consumer Price Index for fuel and utilities faced by the individual household, annual utility expenditures increased \$3.12. Evaluated at the initial year mean values for annual expenditures and price, this coefficient yields an estimate of the price point elasticity of total expenditures equal to .61 $(3.118 \cdot \frac{79}{400})$. Thus, a 10 percent increase in the price of utilities would be

Table 10.12 (page 1 of 3)

REGRESSION RESULTS FOR CHANGE IN UTILITY EXPENDITURES, 1971-1977, HOMEOWNERS IN BOTH YEARS WHO REPORTED SOME UTILITY PAYMENTS IN EACH YEAR
(N=1,823)

Independent Variable (O[illegible]ed Category)	(1)	(2)
Co[illegible]tant	148.25	174.71
Change in Economic Factors		
Change in price	3.078**	3.118**
	(.441)	(.440)
Change in income ($100)	.582**	.478**
	(.094)	(.098)
Change in Weather Conditions		
Change in heating degree days (10 degrees)	2.296**	2.131**
	(.290)	(.292)
Change in cooling degree days(10 degrees)	5.250**	4.901**
	(.739)	(.742)
Change in Characteristics of the Dwelling Unit		
Change in number of rooms	3.768	2.093
	(7.886)	(7.838)
Change in house value ($1000)	3.238**	3.235**
	(.448)	(.448)
(Remained in single-family house)	-	-
Apartment to house	180.74**	156.18**
	(56.37)	(56.07)

Table 10.12 (page 2 of 3)

Independent Variable (Omitted Category)	(1)	(2)
Trailer to house	-129.61+ (78.81)	-133.71+ (78.14)
House to apartment	14.47 (71.97)	38.91 (71.51)
Remained in apartment	117.02* (48.10)	106.89* (47.84)
Trailer to apartment	-308.45 (369.67)	-329.25 (366.34)
House to trailer	-39.87 (75.18)	-26.89 (74.61)
Apartment to trailer	-79.23 (304.11)	-21.55 (301.50)
Remained in trailer	-109.66+ (57.76)	-99.38+ (57.29)
Change in Household Utilization Characteristics		
Change in Family size	-.301 (8.984)	7.292 (9.042)
Change in real home food expenditures ($100)	3.681** (1.405)	3.326* (1.394)
Change in house-work hours	.010 (.010)	.007 (.010)
(Always not self-employed	-	-

Table 10.12 (page 3 of 3)

Independent Variable Omitted Category)	(1)	(2)
Became self-employed	150.31** (44.79)	143.97** (44.49)
Became not self-employed	50.19 (42.77)	66.36 (42.59)
Always self-employed	75.94* (37.93)	76.47* (37.83)
Demographic Characteristics of the Household		
Whether black	-	182.41** (38.49)
Whether elderly head, 1977	-	-83.85** (22.06)
(Always nonpoor)	-	-
Became nonpoor	-	-59.17 (52.36)
Became poor	-	25.31 (66.62)
Always poor	-	-29.40 (65.28)
R^2	.138	.154

Significance levels: **.01 *.05 +.10
Numbers in parentheses are standard errors.

expected to result in a 6.1 percent increase in expenditures on utilities. However, given the substantial change in both price and expenditures over the six-year period, it would seem more appropriate to calculate the arc elasticity of expenditures, that is, use the average of the 1971 and 1977 mean values as the base of comparison. The resulting estimate of the price arc elasticity of expenditures equals .53 $(3.118 \cdot \frac{79 + 137}{400 + 864})$. A 10 percent increase in the price of fuel would be expected to lead to a 5.3 percent increase in annual expenditures, indicating an inelastic price demand for utilities.

For every \$100 increase in annual household money income, annual expenditures on utilities increased approximately \$.48, which translates into an income arc elasticity of total expenditures equal to .128[6] $(.00478 \cdot \frac{13,334 + 20,467}{400 + 864})$. This implies that a 10 percent increase in income would result in a 1.3 percent increase in expenditures on utilities, other factors held constant. The fact that the income elasticity of total expenditures on utilities is substantially less than one is the primary reason that a steeply regressive burden of utility payments was found in the first section of this chapter.

The weather, as expected, significantly affected utility payments. A ten degree increase in heating degree days led to a \$2.13 increase in annual utility payments, while a ten degree increase in cooling degree days resulted in a \$4.90 increase in expenditures. Viewed in another light, the elasticity of total expenditures with respect to heating degree days equalled .93 $(.2131 \cdot \frac{2765 + 2769}{400 + 864})$ the elasticity with respect to cooling degree days was .54 $(.49 \cdot \frac{659 + 724}{400 + 864})$. Thus, a 10 percent increase in colder weather (i.e., weather below 65 degrees Fahrenheit, as measured by heating degree days) would lead to a 9.3 percent increase in utility expenditures, while a 10 percent increase in warmer weather (as measured by cooling degree days) would result in a 5.4 percent increase in expenditures. The fact that utility expenditures are more responsive to percentage changes in cold weather than to changes in warm weather is perhaps not surprising, given that heat is a necessity while airconditioning is more of a luxury. On the other hand, the fact that an absolute change in cooling degree days leads to a greater change in utility expenditures than an absolute change in heating degree days (as indicated by the larger coefficient on cooling degree days) may indicate that airconditioning is relatively more expensive per degree of temperature control than is heating.

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[6] It should be noted that one of the factors controlled for is poverty status, which can be expected to be correlated with income. (The same can be said with respect to race and age.) Thus it could be argued that in calculating the income elasticity it may be more appropriate to use the result in Column 1. Evaluated with this coefficient the arc income elasticity of total expenditures on utilities equals .165.

It was hypothesized that changes in the characteristics of the dwelling unit would affect changes in utility expenditures. To some extent the results supported this line of reasoning. Change in the number of rooms in the residence did not significantly determine the change in utility expenditures, perhaps because of the small variance of the variable. (Mean= .09, Standard Deviation= 1.24, with 55.9 percentage of the sample having no change in the number of rooms in their dwelling unit.) Increases in the value of the residence did lead to larger increases in utility expenditures, with every $1,000 increase in house value resulting in a $3.24 increase in the change in utility expenditures. This result could reflect the fact that larger homes or ones with more luxury-oriented appliances (e.g., central airconditioning), which presumably had the largest absolute increases in value, are more energy expensive.

The type of structure in which the family resides, and changes in such, affected the change in utility payments. Households which switched from a multi-family type structure to a single-family house had a change in utility expenditures which averaged $156 more than the change experienced by the omitted category of households which resided in single-family house in each of the two years. One might think that this results from single-family houses being more expensive to heat, perhaps because they may be larger for a given number of rooms. However, this explanation is somewhat negated by the fact that those families which lived in duplexes, townhouses, or condominiums in both years had increases in their utility expenditures which averaged $107 more than the omitted category. It is not clear why this occurred. Finally, families which resided in trailers in both 1971 and 1977 had increased utility expenditures which averaged $99 less than the increase of single-family homeowners, a coefficient which was not quite significant at the 5 percent level.

Households which utilize their residences more would be expected to have higher utility expenditures, and differential increases in utilization should lead to differential increases in annual utility expenditures. The results provided mixed support for this argument. Changes in family size and changes in housework hours, two variables entered in the equation as measures of changes in utilization, were both insignificant in predicting changes in annual utility expenditures. On the other hand, changes in home food expenditures (in real terms) were positively related to changes in utility expenditures, with a $100 increase in food expenditures resulting in a $3.33 increase in annual utility use. This variable was entered into the equation as a measure of intensity of use of kitchen appliances, with the reasoning that households which increased their home food expenditures (in real terms) were eating more at home and consequently using the kitchen more. As noted above, this is not the ideal variable to measure the desired behavior, as an increase in food expenditures may reflect an upgrading in quality rather than quantity.

As was seen in the descriptive part of this chapter, whether the head of the household is self-employed has a significant effect on the change in utility payments. Households in which the head became self-employed by 1977 had increases in annual utility expenditures which averaged $144 higher than the omitted category of households in which the head was not self-employed in either 1971 or 1977. Households in which the head was self-employed in both 1971 and 1977 had increases which averaged $76 more than the non-self-employed. The probable explanation for the higher increases for self-employed persons is that some utility expenses from their businesses were being included in total utility payments. This could easily occur if the business were run out of the home.

In the descriptive section of this chapter we found that black homeowners were particularly hard hit by rising utility costs. The multivariate results confirm this conclusion. Even when other factors are controlled for, black homeowners had increases in their utility expenses which averaged $182 higher than that of nonblack homeowners. It is not possible from the available data to determine why black homeowners had such considerably higher increases, but two possible explanations suggest themselves. Black homeowners may not be as conservation minded as non-black homeowners, and may, for example, prefer to keep their thermostats at the more comfortable level of 72 degrees Fahrenheit than cutting back to the widely recommended but certainly cooler temperature of 68 degrees Fahrenheit. A more likely explanation is that black homeowners reside in houses which are older and not as structurally sound as the houses of nonblacks, thus making energy-efficient improvements more costly and less likely to be done.

Households headed by a person aged 65 of more in 1977 had significantly lower than average increases in annual utility expenditures. Unfortunately, despite this apparent conservation effort on the part of the elderly, they still experienced above-average increases in the burden of utility expenditures, as was seen in Table 10.11. A somewhat similar fate befell those households which either were in poverty in each of the two years or fell into poverty between 1971 and 1977. The increase in their utility expenditures did not differ significantly from that of the nonpoor in both years, yet the burden of such expenditures increased dramatically.

LINEAR EXPENDITURE FUNCTION

The model. In the above section we assumed no specific functional form for the expenditure function. A conventional use of the expenditure function it to assume that it is linear.[7] Under this assumption we have the following specifications:

$$(3)\ E_{i,71} = \alpha_{i,71} + B_1P_{i,71} + B_2Y_{i,71} + B_3D_{i,71} + e_{i,71}$$

$$(4)\ E_{i,77} = \alpha_{i,77} + B_4P_{i,77} + B_5Y_{i,77} + B_6D_{i,77} + e_{i,77}$$

These cross-sectional estimation equations state simply that utility expenditures of the ith household in a given year are a linear function of prices, incomes, and a set of demographic variables. It should be noted that in the above specification we have allowed the effect of the independent variables on total expenditures to be different in the two years.

To determine the change in utility expenditures between 1977 and 1971, Equation 3 is subtracted from Equation 4, yielding

$$\begin{aligned}(5)\ E_{i,77} - E_{i,71} = {} & \alpha_{i,77} - \alpha_{i,71} + B_4P_{i,77} - B_1P_{i,71} + B_5Y_{i,77} \\ & - B_2Y_{i,71} + B_6D_{i,77} - B_3D_{i,71} + e_{i,77} - e_{i,71}\end{aligned}$$

Adding and subtracting a series of cross-product terms (such as $B_4P_{i,71}$) to the right-hand side of Equation 5 results in the following equation:

$$\begin{aligned}(6)\ \Delta E_i = {} & \alpha_{i,77} - \alpha_{i,71} + B_4(P_{i,77} - P_{i,71}) + (B_4 - B_1)P_{i,71} \\ & + B_5(Y_{i,77} - Y_{i,71}) + (B_5 - B_2)Y_{i,71} + B_6(D_{i,77} - D_{i,71}) \\ & + (B_6 - B_3)D_{i,71} + e_{i,77} - e_{i,71}\end{aligned}$$

or

$$\begin{aligned}(7)\ \Delta E_i = {} & \Delta\alpha_i + B_4\Delta P_i + \Delta B_P P_{i,71} + B_5\Delta Y_i + \Delta B_y Y_{i,71} \\ & + B_6\Delta D_i + \Delta B_D D_{i,71} + e_{i,77} - e_{i,71}\end{aligned}$$

where

ΔB_p = change in the coefficient on the price variable
ΔB_y = change in the coefficient on the income variable
ΔB_d = change in the coefficient for the demograhic variables

In this formulation the coefficients on the change variables represent the new (i.e., the 1977) equilibrium response to changes in the variable of interest while the coefficients on the 1971 variables indicate changes in the equilibrium response over the six-year period.

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[7] A log-linear specification is also commonly assumed. The regression results for a cross-sectional log-linear specification are presented in the Appendix.

To ascertain the effect of changes in categorical variables, a series of dummy variables were used which group households by whether they remained in their initial state or whether they changed from that state. These variables are identical to those used in estimating Equation 2. Consequently, the only difference in estimating Equation 7 compared to Equation 2 is the inclusion of 1971 levels of the continuous variables.

The next section contains a discussion of the results of estimating Equations 3 and 4, the cross-sectional equations, and Equation 6. The discussion is confined to major differences in these results compared to those found when estimating Equation 2. Particular attention is paid to the size of the price and income elasticities.

The Results. Table 10.13 presents the cross-sectional regression results for utility expenditures in 1971 and 1977. In general, factors which were significant in predicting change in the level of expenditures (as seen in Table 10.12) were also significant in predicting the level of expenditures. Two major differences do show up, however. Neither change in the number of rooms in the dwelling unit nor change in family size were significant in determining the change in the level of expenditures. In both 1971 and 1977, however, the number of rooms in the structure was a significant predictor of the level of expenditures. In 1971 an additional room resulted in an additional $18 in expenditures, while in 1977 that figure had doubled to $63. And although in 1971 the number of persons in the household was not a significant determinant of utility payments, in 1977 each additional person in the household added approximately $25 to annual utility bills. In discussing the longitudinal results of the preceding section it was noted that these two attributes of a household showed little change over the six-year period and that this may account for the fact that they do not determine change in the level of payments, even though they might affect the level itself. This seems to be the case.

It is of interest to compare the price and income elasticities derived from cross-sectional results with those found from the longitudinal results. It should be recalled that the arc elasticity of total expenditures with respect to price was found to be .53 and with respect to income to be .13. The cross-sectional results (evaluated at the mean values of the variables of interest) produce estimates which are uniformly lower than those found earlier. The price

Table 10.13 (page 1 of 2)

CROSS-SFCTIONAL RESULTS FOR UTILITY EXPENDITURES,
1971 AND 1977
(For homeowners in both years who reported some utility payments in each year.)

Independent Variable (Omitted Category)	1977 (1)	1977 (2)	1971 (1)	1971 (2)
Constant	-592.59	-626.69	-35.35	-34.68
Economic Factors				
Price	1.712** (.264)	1.684** (.262)	.536* (.220)	.524* (.220)
Income ($100)	.368** (.079)	.385** (.081)	.280** (.050)	.286** (.052)
Weather Conditions				
Heating degree days (10 degrees)	1.340** (.162)	1.394** (.161)	.301** (.073)	.287** (.074)
Cooling degree days (10 degrees)	3.516** (.389)	3.440** (.387)	.437* (.200)	.403* (.200)
Characteristics of Dwelling Unit				
Number of rooms	5.09** (7.99)	36.14** (7.95)	18.37** (3.30)	18.05** (3.30)
House value ($1000)	2.355** (.396)	2.592** (.396)	2.795** (.373)	2.760** (.374)
(Single-family house)	-	-	-	-
Apartment, other	132.41** (39.52)	119.89** (39.33)	15.81 (15.02)	14.56 (15.12)
Trailer	11.68 (46.22)	32.42 (46.08)	101.76** (19.65)	101.01** (19.70)

Table 10.13 (page 2 of 2)

Independent Variable (Omitted Category)	1977 (1)	1977 (2)	1971 (1)	1971 (2)
Household Utilization Characteristics				
Family size	24.24** (7.76)	18.95* (8.09)	3.09 (2.85)	4.75 (2.98)
Home food expenditures ($100)	5.99** (1.01)	6.25** (1.01)	3.23** (.64)	3.25** (.64)
Housework hours	-.003 (.010)	-.000 (.010)	.003 (.004)	.002 (.004)
Whether self-employed	101.25** (29.60)	110.89** (29.48)	24.27* (11.77)	26.79* (11.85)
Demographic Characteristics				
Whether black	-	208.61** (37.69)	-	-3.51 (15.75)
Whether elderly	-	6.13 (23.49)	-	19.55+ (10.49)
Whether poor	-	-19.01 (47.14)	-	-18.04 (17.40)
R^2	.279	.290	.265	.265
N=1,823				

Significance Levels: **.01 *.05 +.10
Numbers in parentheses are standard errors

point elasticity in 1971 was .103 $(.524 \cdot \frac{78.8}{400})$, while in 1977 it had increased to .267 $(1.684 \cdot \frac{136.8}{864})$. The income point elasticity of total expenditures was virtually identical in 1971 and 1977, equaling .095 $(.00286 \cdot \frac{12,343}{400})$ and .091 $(.00385 \cdot \frac{20,467}{864})$ in the respective years. It is often argued that cross-sectional results represent long-run elasticities, while results derived from year-to-year changes indicate short-run elasticities, and this may explain why the cross-sectional elasticities are uniformly lower than the longitudinal elasticities. This point will be discussed more fully below.

Finally, it is worth noting that the cross-sectional results also confirm the extraordinary change undergone by black homeowners with respect to their utility bills. In 1971 they did not differ significantly from white homeowners in the level of their annual utility expenditures. In 1977 black homeowners averaged \$209 more in annual utility expenditures than white homeowners.

The cross-sectional comparisons also show a radical change in the relative cost of owning different types of housing structures. In 1971 families residing in trailers had utility payments which were \$101 less than the omitted category of families living in single-family houses. Families living in apartments did not differ significantly from the omitted group. In 1977 the situation was reversed. Families living in trailers did not differ significantly from those residing in single-family houses, while apartment dwellers averaged \$120 more in annual utility expenditures than single-family residents.

The results from estimating Equation 7, the specification of the change equation derived from the linear expenditure function, are presented in Table 10.14. In general, the same factors which earlier were found to be significant in determining change in annual utility expenditures (see Table 10.12) were also significant when estimating this particular specification of the change equation. This is reassuring, since as was mentioned earlier the only practical difference in the two equations is that the specification of Table 10.14 includes the initial level of the continuous variables while the specification of Table 10.12 does not. Unfortunately, there were enough differences in the two sets of results and also with the longitudinal results versus the cross-sectional results to render a coherent integration of all three difficult.

The responsiveness of annual utility payments to changes in the weather is very similar in the longitudinal results. However, these results differ sharply from the cross-sectional estimates, which show considerably less responsiveness to change in heating and cooling degree days. (However, the relative difference in the effect of warmer weather versus colder weather is approximately the same,

Table 10.14 (page 1 of 4)

REGRESSION RESULTS FOR CHANGE IN UTILITY EXPENDITURES, 1971-1977
(For homeowners in both years who reported
some utility payments in each year.)(N=1,823)

Independent Variable (Omitted Category)	(1)	(2)
Constant	-414.36	-464.24
Change in Economic Factors		
Change in price	2.051** (.738)	1.921** (.735)
Change in income ($100)	.260** (.100)	.265** (.102)
Change in Weather Conditions		
Change in heating degree days (10 degrees)	2.355** (.332)	2.275** (.332)
Change in cooling degree days (10 degrees)	5.396** (.757)	5.115** (.757)
Change in Characteristics of the Dwelling Unit		
Change in number of rooms	11.073 (9.135)	12.636 (9.115)
Change in house value ($1000)	2.781** (.452)	2.925** (.451)
(Remained in single-family house)	-	-
Apartment to house	172.03** (55.94)	154.64** (55.80)
Trailer to house	-88.01 (77.97)	-77.19 (77.67)
House to apartment	49.62 (70.45)	56.90 (70.16)
Remained in apartment	139.43** (47.28)	120.75* (47.19)
Trailer to apartment	-352.04 (361.79)	-329.32 (359.76)

Table 10.14 (page 2 of 4)

Independent Variable (Omitted Category)	(1)	(2)
House to trailer	-15.23 (73.77)	6.26 (73.52)
Apartment to trailer	-4.60 (297.25)	14.58 (295.66)
Remained in trailer	-53.00 (59.09)	-36.41 (58.91)
Change in Household Utilization Characteristics		
Change in family size	35.86** (10.74)	33.82** (10.75)
Change in real home food expenditures ($100)	7.31** (1.75)	7.57** (1.75)
Change in housework hours	.007 (.011)	.007 (.011)
(Always not self-employed)	-	-
Became self-employed	123.74** (44.11)	128.85** (43.90)
Became not self-employed	39.27 (41.97)	43.03 (41.95)
Always self-employed	36.14 (37.64)	43.10 (37.66)

Table 10.14 (page 3 of 3)

Independent Variable (Omitted Category)	(1)	(2)
Original Levels		
1971 price	.219 (.821)	.344 (.817)
1971 income ($100)	.121 (.128)	.134 (.132)
1971 heating degree days (10 degrees)	.761** (.187)	.849** (.187)
1971 cooling degree days (10 degrees)	2.131** (.495)	2.183** (.493)
1971 number of rooms	16.98+ (9.52)	18.40+ (9.50)
1971 house value ($1000)	.363 (.944)	.701 (.942)
1971 family size	21.22* (8.64)	12.59 (9.10)
1971 home food expenditures ($100)	7.60** (2.00)	8.22** (2.00)
1971 housework hours	-.009 (.012)	-.002 (.012)

Table 10.14 (page 4 of 4)

Independent Variable (Omitted Category)	(1)	(2)
Demographic Characteristics		
Whether black	-	193.47** (38.66)
Whether elderly head, 1977	-	-15.61 (24.12)
(Always nonpoor)	-	-
Became nonpoor	-	-22.23 (52.82)
Became poor	-	30.65 (65.68)
Always poor	-	35.56 (64.81)
R^2	.181	.191

Significant levels: **.01 *.05 +.10
Standard errors in parentheses.

with cooling degree days having an absolute effect on utility expenditures which is a little more than twice the size of the effect of heating degree days. On the other hand, the elasticity of total expenditures with respect to changes in heating degree days is larger than the elasticity with respect to cooling degree days.) This result is particularly troublesome in light of the fact that the coefficients for the change variables in the linear expenditure function specification (Table 10.14) are meant to be interpreted as the equilibrium 1977 response of utility payments to changes in the weather. This is also the interpretation to be given to the 1977 cross-sectional coefficients. The results in Table 10.14 indicate that there was a significant change between 1971 and 1977 in the equilibrium response of households to changes in the weather, as evidenced by the significant coefficients on 1971 heating and cooling degree days. This result is supported by a comparison of the cross-sectional coefficients for 1971 and 1977.

In sum, the two specifications of the longitudinal change equation produced similar results with respect to changes in the weather, results which differed in magnitude from the cross-sectional results. A different situation arose from comparisons of the effect of price and income on utility expenditures. With respect to the effect of the price of utilities, the linear expenditure function specification of the change equation yielded results which were similar to those from the 1977 cross section. From the change equation a one percentage point increase in the utility price index resulted in a $1.92 increase in annual utility payments, while from the 1977 cross-sectional results an effect of $1.68 per percentage point increase was estimated. These figures translate into price elasticities of .328 and .267, respectively. (Recall that the longitudinal elasticity is an arc elasticity while the cross-sectional elasticity is a point elasticity.) These results imply that a 10 percent increase in the price of utilities would, on average, result in an increase in annual utility expenditures of between 2.7 percent and 3.3 percent. However, these estimates differ considerably from that derived from estimating Equation 2--the general change equation--which yielded a price (arc) elasticity of .53. (It is also worth noting that the results in Table 10.14 show no significant change between 1971 and 1977 in the equilibrium response to price changes. This is difficult to reconcile with the cross-sectional results from 1971 and 1977, which show a considerable change in the size of the coefficient of the price variable.)

The magnitude of the estimated income elasticity of annual utility expenditures differs in all three specifications, but is uniformly low. From the results in Table 10.12 an income (arc) elasticity of .128 was derived. From the 1977 cross-sectional results in Table 10.13 an income point elasticity of .091 was calculated. Finally, the coefficient on change in income presented in Table 10.14 implies an income (arc) elasticity of annual utility expenditures of .071 $(.00265 \cdot \frac{13,334 + 467}{400 + 864})$. From these results we can conclude that a 10 percent increase in income would lead to an increase in annual utility expenditures of between .7 and 1.3 percent.

A Comment on the Differing Estimates of Elasticity

In Table 10.15 we have summarized the estimates of certain elasticities derived from the different specifications estimated in this chapter. Summarizing the discussion in the preceding section, the two change equations yielded very similar estimates of the responsiveness of annual utility expenditures to changes in the weather. These estimates are approximately twice as large as the responsiveness implied by the cross-sectional results. The estimated income elasticity of annual expenditures did not differ meaningfully in magnitude in the three specifications. The estimates of the price elasticity of annual utility expenditures compared oddly. The two longitudinal estimates differed considerably, with the estimate derived from the linear specification of the expenditure function being very similar to the cross-sectional estimate.

Can these differences be reconciled? It is commonly thought that cross-sectional elasticities measure longer-run responses to variables of interest, while elasticities estimated from year-to-year changes measure shorter-run responses.[8] The rationale behind this proposition is that cross-sectional estimates compare households at different levels of a particular variable at one point in time. If it can be assumed that the cross-sectional differences in these levels measure differences that are reasonably stable over time, then the estimates based on these differences actually show how households respond to long term differences in a particular variable. Estimates based on year-to-year changes, on the other hand, show household responses to short run changes in a particular variable, without sufficient time to implement any longer run responses which might occur.

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[8] This argument is usually advanced when comparing cross-sectional elasticities to elasticities estimated from aggregate time-series data.

Table 10.15

ESTIMATES OF VARIOUS ELASTICITIES OF ANNUAL UTILITY EXPENDITURES FROM DIFFERENT SPECIFICATIONS OF THE ESTIMATION EQUATION

Elasticity of Annual Utility Expenditures with Respect to:	Specification Upon Which Estimate is Based: General Expenditure Function Specification of Change Equation (Table 10.1)	1977 Cross-sectional Specification* (Table 10.13)	Linear Expenditure Function Specification of Change Equation (Table 10.14)
Price	.53	.27	.33
Income	.13	.09	.07
Heating degree days	.93	.45	1.00
Cooling degree days	.54	.29	.56

*These elasticities are point elasticities, based on 1977 mean values of the variables of interest. All others are arc elasticities, based on the average of the mean values for 1971 and 1977.

This line of reasoning may help explain the difference between the cross-sectional and longitudinal estimates of the elasticity of expenditures with respect to changes in the weather.

It is reasonable to assume that areas of the country which have relatively colder weather one year would have relatively colder weather other years, although the intensity of the coldness would vary from year to year. North Dakota will almost always have colder winters than Alabama, although the severity of the difference may vary annually. It also seems reasonable to believe that residential structures are built with an eye toward the normal weather conditions of an area. We would expect houses in North Dakota to be better insulated against cold weather than houses in Alabama. It might be argued that the cross-sectional results reflect the effects of these facts on annual utility expenditures. By definition, the cross-section shows in a given year how utility payments varied across households in different localities which experienced different weather conditions. The longitudinal results show how household utility expenditures respond over time to flucuations in weather conditions around the normal pattern.[9] Given this, we might interpret the results to mean that, if a particular area of the country experienced 10 percent colder weather in one year as compared to another, annual utility expenditures would be expected to increase 9 or 10 percent, based on the longitudinal elasticity of .93 or 1.00, depending on the specification. On the other hand, if the family moved to an area which normally experienced 10 percent colder weather, annual utility expenditures would be expected to increase only 4.5 percent (the cross-sectional elasticity), because they would be moving to a better insulated house. To put it another way, if a family packed up its belongings and moved to another house in an area with 10 percent colder weather, its annual utility expenditures would increase 4.5 percent (other factors held constant); if that family moved its house to the new location, its annual utility expenditures would increase by 9 or 10 percent.

It is more difficult to reconcile the differing price elasticities, but perhaps a similar line of reasoning would apply. Although utility prices rose sharply throughout the nation in the six-year period between 1971 and 1977, it is probably true that relatively high price areas in 1977 had relatively high prices in 1971 as well. The cross-sectional elasticity of .27 could reflect these longer-run differences, while the longitudinal elasticity of .53 reflects the effect of the rising prices over the six-year period.

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[9] This interpretation is clouded somewhat by the fact that some families moved to different localities in the six-year interval.

One glaring difficulty in interpreting these results is that all the specifications assume households are in their long-run equilibrium position with respect to utility expenditures. Given the rapidly changing energy picture, especially for prices, households are probably in the process of moving toward a longer-run equilibrium position. Thus, the next step in extending the analysis would specify a partial adjustment model to see how that changes the results.

SUMMARY AND CONCLUSIONS

We have seen a lot of numbers in this chapter, but the major findings can be stated succintly.

First, there can be little doubt that low income households have been particularly hard hit by the dramatic increase in utility prices between 1971 and 1977. In 1977, 31.1 percent of all households in the lowest income decile spent more than 20 percent of their income for utilities, compared to 14.8 percent in 1971. Virtually no households in the highest income decile spent more than 10 percent of their incomes on utility payments in either 1971 or 1977. Even these figures fail to show the full extent of the burden on low income households. In 1977, fully one-third of the households in the lowest income decile reported _no_ utility payments, presumably because such payments were included in their rent. It is unrealistic to think that they were unaffected by rising utility costs. Only 3.0 percent of the households in the highest income decile reported no utility payments in 1977. When this problem is eliminated by looking at homeowners only, the disparity in burdens between high and low income households is even more pronounced. Almost 60 percent of the homeowners in the lowest income decile in 1977 had to devote over 20 percent of their income to utility payments. _No_ homeowners in the two highest deciles reported utility payments which exceeded 10 percent of their income.

Second, black homeowners experienced exceptionally large increases in their annual utility payments between 1971 and 1977. While nonblack homeowners had increases which averaged $453 over the six-year period, black homeowners had increases which averaged $619. This large differential remained even when other factors, such as the weather, were controlled for in a multivariate regression analysis. This large increase was the main reason why the percentage of income directed to utility payments increased substantially more for black homeowners (5 percentage points) as compared to nonblack homeowners (2 percentage points).

Third, the poor and the elderly were also hard hit by rising utility prices. The percentage of income devoted to utility payments by households headed by elderly persons increased by 3.6 percentage points between 1971 and 1977, compared to 1.6 percentage points for nonelderly households. This increase occurred despite the fact that the average increase in annual utility payments for elderly homeowners was $356 compared to $514 for nonelderly homeowners. (On an adjusted basis, the increase in utility expenditures of elderly homeowners did not differ significantly from that of nonelderly homeowners). The poor faced a similar situation. Although their utility expenditures increased less rapidly than the nonpoor, the burden of such expenditures increased substantially more.

Fourth, as predicted, income, price, the weather, characteristics of the dwelling unit, and changes in these factors, were all significant in predicting both the level and the change in utility expenditures. The longitudinal estimate of the price elasticity of expenditures was .53, indicating that a 10 percent increase in the price of utilities results in a 5.3 percent increase in expenditures. The cross-sectional estimate of the price elasticity was considerably lower, equalling .27. Various estimates of the income elasticity of total expenditures were uniformly low, equalling approximately .10. This low income elasticity, not unexpected regarding a product which is a basic necessity, is the primary reason for the steeply regressive pattern of the burden of utility payments. The elasticity of utility expenditures with respect to cold weather (i.e., heating degree days) was substantially higher than the elasticity with respect to warmer weather (i.e., cooling degree days). Longitudinal estimates of the elasticity with respect to heating degree days equalled approximated 1.00, indicating that a 10 percent increase in cold weather would result in a 10 percent increase in utility expenditures. The longitudinal estimates of the elasticity with respect to cooling degree days equalled about .5. The comparable cross-sectional elasticities were about one-half of the longitudinal estimates, indicating that utility expenditures were probably less responsive to inter-area differences in weather than changes within an area. This could result from residential structures being built according to the normal weather pattern in a given area.

In sum, the results of this chapter provide some interesting insights into the determinants of utility expenditures and on how the burden of rising utility bills has hit particular subgroups of the population. Two points should be noted. One, it does appear that households have cut back their use of energy in response to higher prices. Although the price elasticity of expenditures is positive, it is considerably less than one, indicating that as the price increases (other factors held constant) the quantity of energy consumed (as measured by annual utility expenditures) decreases. Furthermore, the lower cross-sectional elasticities of expenditures with respect to the weather (compared to the longitudinal elasticities) may indicate that there is room for further conservation resulting from more energy-efficient residential structures. This statement is predicated on the reasoning advanced in the text, that the lower cross-sectional elasticities reflect the fact that residential structures are built to suit the longer-run weather conditions of an area. This implies that if houses in warmer areas of the country were built more like houses in colder areas of the country (in terms of heating efficiency), a reduced use of energy for home heating would result.

This optimism, however, must be tempered by the fact that certain segments of our population are suffering considerably as a result of the rapid increase in utility prices. Blacks, the poor, and the elderly have all experienced substantial increases in the burden of utility payments. In the face of ever-increasing energy costs, it would seem imperative that some form of aid be granted to these groups to enable them to cope with their burden.

References

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Appendix Table A10.1 (page 1 of 2)

LOGARITHMIC SPECIFICATION OF CROSS-SECTIONAL MODEL OF LEVEL OF UTILITY PAYMENTS, 1971 AND 1977

(Homeowners in both years who reported some utility payments in each year.)
(N=1,823)

Independent Variable (Omitted Category)	Dependent variable: LN Annual Utility Expenditures 1977	1971
Economic Factors		
LN fuel price	.288** (.045)	.089+ (.046)
LN income	.109** (.021)	.082** (.020)
Weather Conditions		
LN heating degree days	.348** (.035)	.106** (.036)
Cooling degree days	.282** (.030)	.018 (.035)
Characteristics of Dwelling Unit		
LN number of rooms	.302** (.049)	.251** (.045)
LN house value	.052** (.019)	.120** (.018)
(Single family home)	-	-
Apartment, other	.107* (.044)	.031 (.038)
Trailer	-.026 (.055)	.280** (.052)

Appendix Table A10.1 (page 2 of 2)

Independent Variable (Omitted Category)	Dependent variable: LN Annual Utility Expenditures	
	1977	1971
Household Utilization Characteristics		
Family size	.024** (.008)	.018** (.007)
LN home food expenditures	.125** (.025)	.100** (.024)
Whether self-employed	.109** (.033)	.076** (.029)
Demographic Characteristics		
Whether black	.205** (.042)	-.040 (.039)
Whether elderly	.019 (.028)	.047+ (.028)
Whether poor	.090 (.058)	-.041 (.049)
R^2	.311	.253

Significance levels: **.01 *.05 +.10
Note: The variable "housework hours" has not been included in these regressions.

Chapter 11

THE RELATIVE BURDEN OF HIGHER GASOLINE PRICES*

Daniel H. Hill

Shortly after the oil embargo of 1973-74, Holmes (1975) utilized the data from the Panel Study of Income Dynamics to estimate the relative burden of increased gasoline prices on the American public. In performing this analysis, he assumed a fleet average fuel economy of 13 miles per gallon and an average price of 55 cents per gallon. His conclusions were that, with the exception of a small number of working poor who commuted a great distance, the impact of increased gasoline prices was progressive in that the burden increased proportionately to the ability to pay. This conclusion was the result of the fact that both vehicle ownership and miles driven are highly income elastic. Holmes tempered his findings by noting that public policymakers should be sensitive to the existence of a small minority of households that would be severely burdened by the increased gasoline prices.

In the first section of this paper we shall re-examine these findings, using information obtained in the 1978 interviewing year of the Panel Study of Income Dynamics. This information relates to the driving behavior of a representative sample of American households for the calendar year 1977. Additionally, we address the question of responsiveness of households to increased gasoline prices. Specifically, in the first section we examine the question of whether families who were heavily burdened by gasoline prices in 1973 remained so four years later. In the second section of this chapter we examine the determinants of change in driving behavior. In the final section we employ a computer search algorithm to isolate any possible subgroups of the population that are severely burdened by high gasoline prices.

THE RELATIVE BURDEN OF GASOLINE PRICES IN 1977

Table 11.1 presents the annual miles driven in 1977 by members of the 6,154 households which comprised the sample. Slightly more than 16 percent of these families did not drive at all and did not own an automobile. This percentage

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represents a slight decrease in vehicle ownership over the period from 1973 through 1977. This decrease, however, was more than offset by the increase in miles driven by vehicle-owning households and by the 8.5 percent increase in the total number of households over the period. The percentage of families who reported driving more than 20,000 miles per year increased by more than 10 percent.

Table 11.1

TOTAL MILES DRIVEN IN 1977
(For all Families.)

Annual Mileage	Unweighted Number of Cases	Weighted Percent
Zero, Don't own car	1411	16.3
1 - 5,000	703	11.3
5,000 - 10,000	702	12.5
10,000 - 15,000	904	16.7
15,000 - 20,000	637	12.1
20,000 - 25,000	583	10.5
25,000 - 30,000	321	5.8
30,000 - 35,000	295	5.1
35,000 or more	432	7.6
NA or Don't Know	166	2.0
	6154	100.0

Because so much of the development of the United States has occurred since the turn of the century, and because this period has been characterized by abundant and inexpensive petroleum, the geographic pattern of residence and employment in the United States differs considerably from that in other countries. The typical American worker lives considerably further from his job than his European counterpart and does not have as ready access to public transportation. Employment, therefore, implies certain commuting costs which can be expected to rise in the short and intermediate run as gasoline prices increase. Of all the reasons for driving, the trip to work is probably the least discretionary. As Table 11.2 indicates, however, commuting miles represent only a small fraction of total miles driven by U.S. families. Commuting to work represented 50 percent or more of the total miles driven for less than 8 percent of the population. This percentage varied considerably across income deciles, from 1.4 percent in the lowest income decile to a high of 12 percent in the seventh decile. In contrast, commuting miles accounted for less than 10 percent of total miles for slightly more than 44 percent of the population in addition to the 18 percent of the population who did not own cars. The income responsiveness of car ownership shows up vividly in the first column of Table 11.2. Over three-fifths (60.2 percent) of the households in the lowest income decile owned no cars, while only one percent of those in the highest income decile were carless.

In his paper using the 1973 Panel Study data, Holmes assumed an average fuel economy of 13 miles per gallon and an average price of regular gasoline of 55 cents per gallon in analyzing the burden of increased gasoline prices. In 1977, the corresponding quantities were 14 miles per gallon (the increase was due primarily to increased shares of smaller and lighter cars) and 65 cents per gallon for unleaded regular gasoline. Table 11.3 presents the mean gasoline expenditure in 1977 as a percentage of total family income for the 5,977 households in the Panel Study who reported total miles driven. Overall, 4.3 percent of total family money income was allocated to gasoline expenditures--a figure which is slightly less than that calculated by Holmes. As was the case in Holmes's original analysis, this percentage generally decreased with family money income. Households in the lowest tenth of the income distribution spent, on average, 4.9 percent of their income for gasoline. The fact that only 32 percent of these households owned a vehicle at all, however, implies that for those who did own vehicles the average expenditure was more than 12 percent of their total money resources. For persons in the highest income decile, 2.7 percent of total family money income was allocated to gasoline expenditures. Table 11.4 presents the distribution of mean gasoline expenditures within income deciles. Of the families in the lowest income decile, 9.2 percent allocated more than 20 percent of their income toward gasoline. Only 1.2 percent of the households with incomes which placed them in the upper half of the income distribution spent more than 20 percent of their income on gasoline. Indeed, nearly 90 percent of the households in the highest income decile spent less than 5 percent of their income on gasoline. Thus, Table 11.4 confirms the fact that, while the overall impact of increased gasoline prices falls most heavily on those most able to pay, there are

Table 11.2

VARIATION BY INCOME DECILE IN COMMUTING*/TOTAL MILES, 1977
(For all families.)

Family Income Decile - 1977	No Car	Commuting/Total Miles 0-10 Percent	10-20 Percent	20-30 Percent	30-40 Percent	40-50 Percent	>50 Percent	Don't know	Total
Lowest	62.0	32.3	1.7	0.2	0.5	0.5	1.4	1.3	100.0
Second	43.6	45.5	1.8	1.6	1.2	1.0	2.7	2.7	100.0
Third	22.0	52.6	5.5	5.1	3.6	2.4	4.4	4.3	100.0
Fourth	12.7	48.6	8.4	4.5	3.9	3.9	11.5	6.1	100.0
Fifth	8.4	47.0	12.2	7.5	4.5	6.1	10.2	4.2	100.0
Sixth	4.6	47.8	11.9	9.7	9.0	4.9	8.7	3.4	100.0
Seventh	2.8	39.4	13.0	12.7	8.7	7.8	12.0	3.7	100.0
Eighth	0.8	38.4	18.4	10.2	12.1	6.7	9.8	3.6	100.0
Ninth	1.0	43.6	17.1	12.7	9.7	5.3	7.9	2.6	100.0
Highest	1.1	45.5	20.3	8.7	7.9	7.1	7.5	1.9	100.0
All	15.9	44.1	11.0	7.3	6.1	4.6	7.6	3.4	100.0

Number of Cases: 6154

*Refers to annual commuting miles of heads of households who drive to work.

still a number of relatively poor households for whom the increased gasoline prices represent a substantial burden.

Table 11.3

MEAN GASOLINE EXPENDITURES AS A PERCENTAGE
OF TOTAL FAMILY MONEY INCOME—1977
(For all families.)

Family Money Income Decile	Unweighted Number of Cases	Average Fuel Cost/ Family Money Income
Lowest	730	4.9%
Second	657	3.5
Third	642	5.2
Fourth	639	5.2
Fifth	600	4.6
Sixth	614	4.8
Seventh	573	4.2
Eighth	560	3.9
Ninth	506	4.0
Highest	436	2.7
Total	5977	4.3%

Any policy designed to alleviate this increased burden should be responsive not only to the level of burden but also to its permanency. If high burden is transient--as are catastrophic medical bills for example—then a small scale emergency relief program, such as has been created for utility bills by certain states, would be adequate to handle the problem. If, on the other hand, those people who are heavily burdened by gasoline prices are unable or unwilling to change their driving behavior, then a more in-depth consideration of the problem must be undertaken before a rational policy can be formulated.

Table 11.5 presents a comparison of the relative burden in 1973 (using Holmes' assumed gasoline price measure) and in 1977 for households headed by the same individuals throughout the period. Over 11 percent of the sample were completely unburdened by the increased gasoline prices because they did not own a

Table 11.4

RELATIVE BURDEN OF HIGHER GASOLINE PRICES
BY FAMILY INCOME DECILE
(For all families.)

Family Income Decile	Fuel Cost as Percentage of Income							
	No Car, 0 Percent	0–5 Percent	5–10 Percent	10–15 Percent	15–20 Percent	> 20 Percent	N/A	Total
Lowest	62.0	15.9	5.7	3.5	1.7	9.2	2.1	100.0
Second	43.6	31.1	11.3	5.6	2.8	2.2	3.8	100.0
Third	22.0	36.4	23.9	7.9	2.6	3.4	3.6	100.0
Fourth	12.7	47.2	27.3	7.0	1.6	3.0	1.1	100.0
Fifth	8.4	56.3	23.8	7.9	1.1	0.6	2.0	100.0
Sixth	4.6	55.7	30.6	4.7	1.0	0.4	2.9	100.0
Seventh	2.8	63.7	28.8	3.5	0.1	0.0	1.1	100.0
Eighth	0.8	75.1	20.6	2.1	0.2	0.2	0.9	100.0
Ninth	1.0	75.3	18.9	2.3	1.0	0.0	1.6	100.0
Highest	1.1	89.2	8.7	0.3	0.0	0.0	0.6	100.0
All	15.9	54.6	20.0	4.6	1.3	1.6	2.0	100.0

Number of cases 6154

car in either year. Nearly half of the households (47.7 percent) expended less than 5 percent of their income on gasoline in both 1973 and 1977. Of the 64.7 percent of this sample who fell in the category of expending between zero and 5 percent of their total family income on gasoline in 1973, 2.2 percent had disposed of their automobiles by 1977, while 10.9 percent had increases in burden which moved them up to the next category. The most interesting aspect of the table is that 75 percent of the households (headed by the same persons all five years) which expended more than 10 percent of their income for gasoline in 1973 had managed in one way or another to reduce this burden to less than 10 percent by 1977. While the majority of these households still had appreciable gasoline expenditures, nearly 15 percent of those heavily burdened in 1973 no longer owned a vehicle by 1977.

ANALYSIS OF CHANGE IN GASOLINE EXPENDITURE

The data presented in Table 11.5 suggest that between 1973 and 1977 there was considerable variation in the proportion of family income devoted to gasoline expenditures. These changes in burden were the joint product of changes in gasoline prices, miles driven, and changes in income. Since our primary concern is in understanding the extent to which households alter their driving behavior in response to changes in price and income, we shall pay particular attention to changes in miles driven over the panel period. Because the factors influencing changes at the exterior margin (i.e., selling off all the family cars, or going from being carless to buying a car and driving it for a substantial number of miles) would likely be very different from changes at the interior margin (i.e., changes in the number of miles driven for households who owned cars throughout the period), these changes must be analyzed separately. In this report our attention is almost entirely confined to changes at the interior margin.

So as not to obscure the adaptive behavior of stable economic units with noise introduced by radical changes in household composition, we eliminated from our sample households newly formed by children splitting off to form their own family units and retained only those households with the same heads or spouses in both 1973 and 1977. Figure 11.1 breaks down the change in gasoline burden between 1973 and 1977 into its various definitional components. This burden in any one year was defined as that year's ratio of expenditures on gasoline to income and, therefore, the natural logarithm of this ratio can be expressed as the difference in the natural logarithms of expenditure and income. Similarly, the natural logarithm of the ratio of burden in any two years is equal to the

Table 11.5

GASOLINE EXPENDITURE BURDEN ACROSS YEARS
(For households headed by the same persons, 1973-1977.)

Gasoline Expenditure as Percent Income 1977			Gasoline Expenditure as Percent Income 1973				
	No Car Percent	< 5 Percent	5-10 Percent	10-15 Percent	15-20 Percent	20 + Percent	All
No Car	11.1	2.2	0.3	0.1	0.1	0.7	14.5
< 5 Percent	1.5	47.7	5.9	1.0	0.1	0.9	52.3
5 - 10 Percent	0.7	10.9	6.1	1.0	0.6	0.6	19.9
10 - 15 Percent	0.1	2.0	1.5	0.4	0.3	0.2	4.5
15 - 20 Percent	0.1	0.6	0.2	0.1	0.0	0.1	1.1
20 + Percent	0.4	1.3	0.6	0.2	0.1	0.2	2.8
All	13.9	64.7	14.8	2.8	1.2	2.7	100.0

difference in the logarithm of the ratio of expenditures and the logarithm of the ratio of incomes in those years. The relative importance of these two components of change in accounting for burden is expressed by the standardized regression coefficients obtained when the logarithm of the ratio of burden is regressed on them. The .9561 coefficient between the logarithm of the ratio of expenditure and the logarithm of the ratio of burden in Figure 11.1 can be interpreted as saying that a one standard deviation increase in the ratio of expenditures between 1973 and 1977 is associated with a .9561 standard deviation increase in the ratio of the burden in 1977 and the burden in 1973. Similarly, the -.4522 coefficient between ln(income 1977/income 1973) and ln(burden 1977/burden 1973) means that each standard deviation increase in the ratio of incomes is associated with a reduction in the ratio of burdens of roughly one-half a standard deviation.[1] The relative importance of changes in expenditure in determining burden is therefore roughly twice that of changes in income. Another way of interpreting this is that the ratio of expenditures on gasoline between 1977 and 1973 for panel households was twice as variable as was the corresponding ratio of income and therefore twice as important in accounting for the variance in the ratio of burdens between 1977 and 1973.

Just as change in burden can be decomposed into change in expenditures and change in income, change in expenditures themselves can be broken down into change in gasoline prices between 1973 and 1977 and change in miles driven over the same period. Despite the fact that gasoline prices increased considerably over these five years (rising from an average of 38.4 cents per gallon in 1973 to more than 68 cents per gallon in 1977), the variation in change in gasoline prices was so small compared to the variation of change in actual miles driven as to be almost negligible in determining changes in expenditures on gasoline. The most important component in determining the changes in the fractions of family income spent on gasoline during the period 1973 through 1977 was change in the total number of miles driven by household members. The second most important component was change in income, and change in gas prices was third.

In addition to identifying the definitional relationships between the various components and subcomponents of change in gasoline burden (depicted by the heavy lines in Figure 11.1), it is possible to identify the more interesting behavioral associations in a path analytic framework. In order to do this, one

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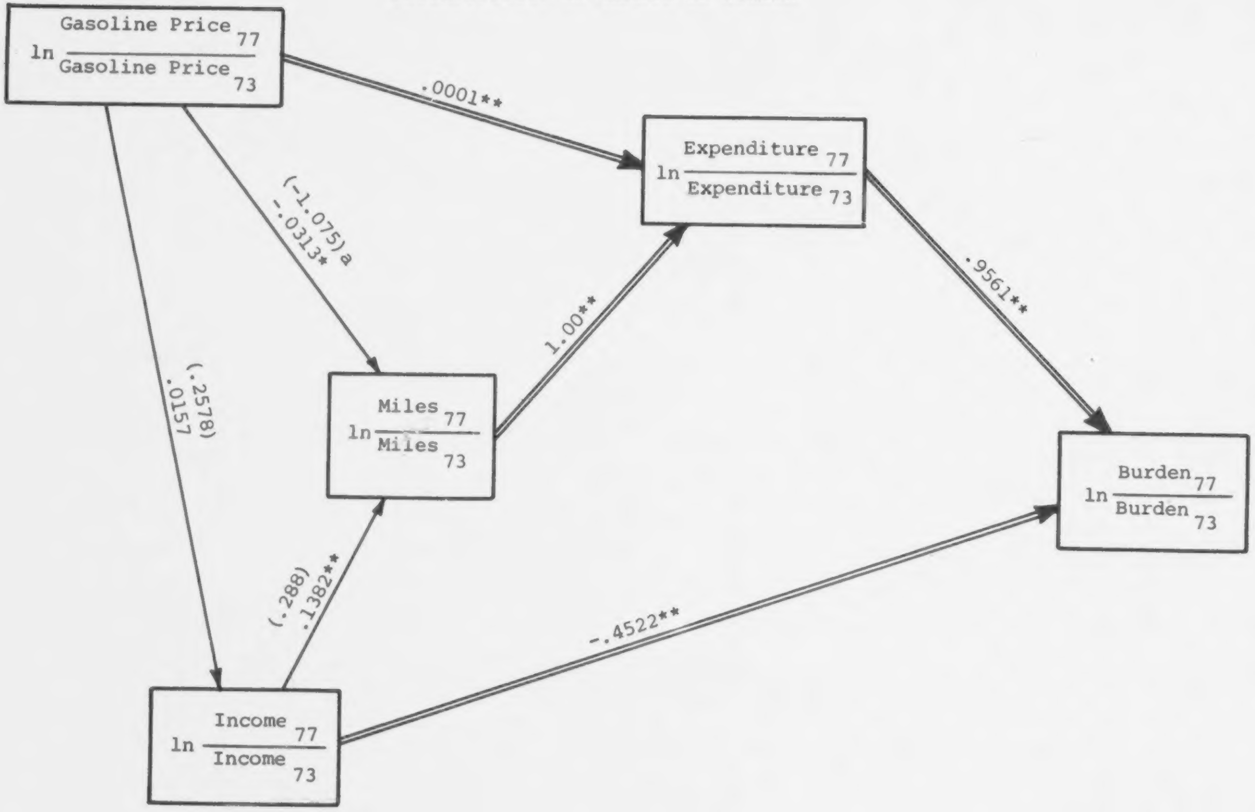
[1] The fact that the coefficients on these components of variance add to something less than unity reflects the fact that the covariance of income and expenditure is positive.

Figure 11.1

DECOMPOSITION OF CHANGE IN BURDEN


Gasoline Price 77
ln
Gasoline Price 73
Expenditure 77
ln
Expenditure 73
Miles 77
ln
Miles 73
Income 77
ln
Income 73
Burden 77
ln
Burden 73
.0001**
.9561**
(-1.075)a
-.0313*
1.00**
(.2578)
.0157
(.288)
.1382**
-.4522**

** Significant at the 99% level of confidence. * Significant at the 95% level of confidence.
Boldface arrows indicate definitional relationship. Lightface arrows indicate behavioral relationship.
[a] Numbers in parentheses are raw score coefficients.

must establish a causal relationship among the variables. In the present instance it seems reasonable to assume that level and change in gasoline prices are completely beyond the control of the individual and therefore are logically prior to variables such as income and miles driven. On the other hand, miles driven in any given year are more discretionary than income and therefore lie causally after income and price. The positioning of the variables in Figure 11.1 and the paths between them show a causal ordering of the following nature. Changes in prices and income have direct effects upon miles driven. There is also a possible indirect effect of price changes on miles driven via income. If the local economy is affected by the level and change of fuel prices and/or availability, then the rate of growth of local economies and hence personal incomes may be negatively associated with the level and rate of increase in prices.

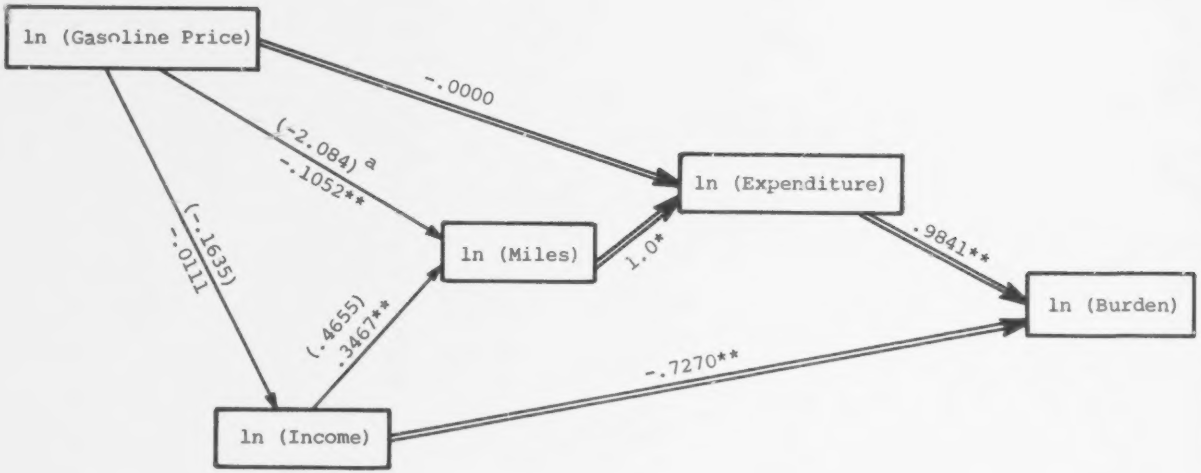
Because all of the five-year change measures are specified in terms of natural logarithms, the nonstandardized regression coefficients can be interpreted as intermediate run (five-year) elasticities. Thus, the intermediate price elasticity of miles driven with respect to gasoline prices is given by the sum of the direct and indirect effects expressed in nonstandardized scores (presented in parentheses in Figure 11.1 above the standardized scores). These figures indicate that, over a five-year period, the gasoline price elasticity of miles driven is slightly larger than one, while the income elasticity is positive and about one-fourth as large as the price elasticity. Equation 1 presents the regression equation from which these elasticities are derived. The constant of .4972 can be interpreted as implying that in the absence of increases in gasoline prices and incomes over the period we would have expected Exp(.4972) = 1.644 ratio of miles driven in 1977 to miles driven in 1973, or a 64 percent increase in miles driven for households in the interior margin. This 64 percent increase, however, was more than offset by the 64 percent increase in gasoline prices over the period and the slight (5.5 percent) increase in miles driven over the period is largely a reflection of the 36 percent increase in income.

$$(1)\quad \ln \frac{\text{Miles}_{77}}{\text{Miles}_{73}} = .4972 - 1.0755 \frac{GP_{77}}{GP_{73}} + .2880 \ln \frac{\text{Income}_{77}}{\text{Income}_{73}}$$

In order to assess the extent to which these five-year elasticities are representative of the long-run effects of the increases in gasoline prices and incomes, it is useful to examine the cross-sectional counterpart of the model presented in Figure 11.1. Figure 11.2 presents both the decomposition of burden

Figure 11.2

1973 CROSS-SECTIONAL DECOMPOSITION


ln (Gasoline Price)
-.0000
(-2.084) a
-.1052**
(-.1635)
-.0111
ln (Expenditure)
ln (Miles)
1.0*
.9841**
ln (Burden)
(.4655)
.3467**
-.7270**
ln (Income)

**Significant at the 99% level of confidence. * Signigicant at the 95% level of confidence.
Boldface arrows indicate definitional relationship. Lightface arrows indicate behavioral relationship.
[a] Numbers in parentheses are raw score coefficients.

in 1973 into its definitional components as well as the cross-sectional income in price elasticities of miles driven in 1973. It is often argued that when inter-area price and income differentials have persisted for some time, such cross-sectional elasticities represent the long-run price and income elasticities. While there has been some differential rate of growth of income across the states since the end of World War II in general, these differentials have been small and it seems fair to assume that the elasticities presented in Figure 11.2 are representative of the order of magnitude of the long-run price and income elasticities. Assuming a linear lag adjustment system, the fact that the cross-sectional price elasticity (-2.16) is slightly greater than twice the intermediate-run elasticity obtained from the five-year longitudinal analysis suggests that the full response to the price shock experienced between 1973 and 1977 would require ten years or more to work itself out. The speed with which households appear to be able to respond to income changes seems to be slightly greater--apparently requiring only about eight years to approach the long-run value of .46.

The implication of the longitudinal analysis that miles driven would have increased by 64 percent over the 1973 to 1977 period if gasoline prices had not increased requires some very careful examination. While the behavioral relationship estimated in Equation 1 was not intended to represent a fully specified behavioral model, it does resemble the type of addilog demand system estimated by Prais and Houthakker (1955). In understanding the demand for any particular commodity, demand systems analysts have found it useful to distinguish between that portion of demand which is responsive to economic variables such as price and income and that which is not. The latter demand is often referred to as the subsistence quantity. This is assumed to be determined by factors exogenous to the particular market being examined. In order to make some of these factors endogenous we now present an expanded left-hand portion of the systems presented in Figures 11.1 and 11.2. In performing this expansion we have tried to identify both those factors which are relevant to the demand for gasoline (or miles driven) and their relative position in a recursive system.

Variables such as gasoline prices and age of the respondent are considered to be totally beyond the control of the individual and, therefore, are causally prior to all other factors. Between these variables and the final outcome variable (logarithm of miles driven or change in logarithm of miles driven) are a number of intervening variables which themselves affect the outcome but which can also be affected by variables at preceding logical levels. Three sets of such variables which are logically subsequent to gasoline prices relate to residential location decisions (existence of public transportation, size of the city, and distance to the center of the nearest city) and labor force commitment variables (weeks worked by head of the household and weeks worked by spouse) and family composition (number of adults and children). Total family money income is considered to be antecedent to all these variables, but income can affect the final outcome variables both directly and via the demand for automobiles and services provided outside the home such as restaurant meals. This recursive ordering is presented graphically in Figure 11.3 below, where position in the causal ordering runs from left to right:

Figure 11.3

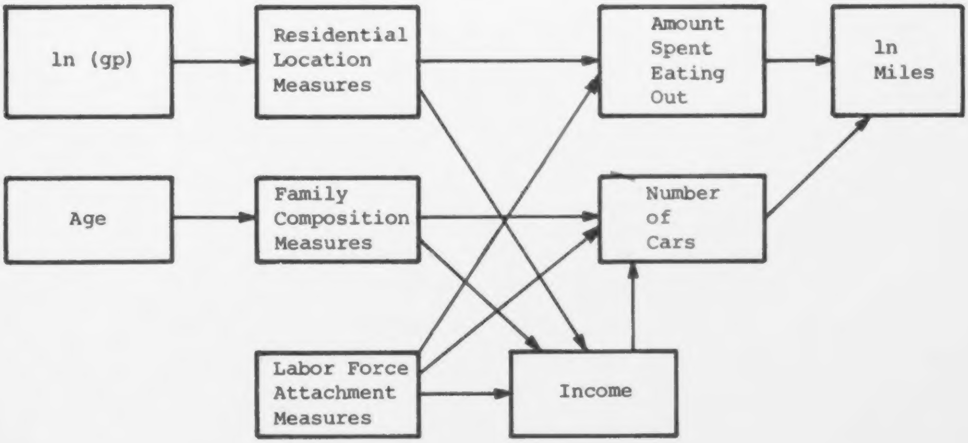

ln (gp)
Residential Location Measures
Amount Spent Eating Out
ln Miles
Age
Family Composition Measures
Number of Cars
Labor Force Attachment Measures
Income

Table 11.6 gives the regression coefficients (and their standard errors) showing both the direct and total effects of the variables presented in Figure 11.3 in determining the driving behavior in 1973. The direct effect of gasoline price in 1973 indicates roughly the same -2 price elasticity which was indicated in Figure 11.2.[2] Aside from the gasoline prices, the most powerful variables in determining the miles driven in 1973 were income, age, number of cars owned, the existence of public transit, and the weeks worked measures. Additionally, a measure of the extent to which the household relied on market services as opposed to home services (as indicated by the logarithm of the annual dollars spent eating out) as well as distance to the center of the city and number of adults were also important and significant determinants of miles driven in 1973. Each percentage increase in family income directly increased miles driven by .2715 percent. But because income also had positive effects on both number of cars owned and on our measure of market orientation (both of which were also positively and significantly related to miles driven) the total effect of a one percent increase in income was a .3255 percent increase in miles driven. Public transportation within walking distance decreased miles driven by .183 percent. Almost all of this effect operated directly on the dependent variables. In contrast, the effect of city size operated indirectly, primarily through income and to a lesser extent through market orientation.

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[2] The difference is due to the adjustment, in this analysis, for spurious correlations resulting from the correlation of age with gasoline price.

Although the finding that households living further away from the center of a large city drove fewer miles than households living nearer the center seems somewhat paradoxical, it is consistent with the findings of others that the further people live from a metropolitan area the more likely it is that their jobs and shopping behavior will be locally oriented.

The labor force attachment variables (logarithm weeks worked by head and logarithm weeks worked by spouse) had direct effects, roughly one-half the size of their total effects and each had a positive and significant impact on miles driven in 1973. The indirect effects for these variables worked almost exclusively through their positive effects on income and, hence, on market orientation and number of cars owned. Similarly, the natural logarithm of number of adults in the household also had a strong positive total effect on miles driven (each one percent increase in number of adults in the household had a total effect of increasing miles driven by .2395 percent), a figure which is three times as large as its direct effect. The other family composition variable, logarithm of number of children had neither significant, direct, nor indirect effects on number of miles driven in 1973.

The final non-economic variable affecting miles driven in 1973 is age, which had a stronger negative direct effect than total effect. The reason is that young people tended to drive more than older ones, but they also had lower incomes and fewer automobiles.

While these cross-sectional results demonstrate the sensitivity of miles driven in a given year to the outcome of decisions regarding residential location

Table 11.6

DIRECT AND TOTAL EFFECTS OF FACTORS INFLUENCING MILES DRIVEN 1973
[Dependent Variable: ln(Miles Driven 1973)]

Variable Name	Direct	Total
Market Orientation	0.138* (0.0054)	0.138* (0.0054)
ln(Number of Cars Owned)	0.3255** (0.0491)	.3255** (0.0491)
Ln(Income)	0.2715** (0.0290)	.3371** (0.0269)
Whether Public Transit	-0.1831** (0.0308)	-.1885** (.0316)
ln(Miles to Work)	0.0646** (0.0121)	.0894** (.0123)
ln(City Size)	-0.0120 (0.0085)	.0085 (.0086)
ln(Weeks Worked)	0.0333** (0.0110)	.0758** (0.0108)
ln(Weeks Worked Spouse)	0.0124 (0.0079)	.0326** (0.0080)
ln(Miles to City)	-0.0363* (0.0161)	-.0446** (0.0165)
ln(Number of Adults)	0.1219** (0.0432)	.3495** (.0403)
ln(Number of Children)	0.0103 (0.0237)	.0103 (0.0241)
ln(Age of Head)	-0.098** (0.0010)	-.0140*** (0.0009)
ln(Gasoline Price)	-2.0393** (0.3081)	-2.2161** (0.3341)
N	3232	3232
R^2	.229	INAP

and family structure, the fact remains that most of the effect of gasoline price in determining miles driven operated directly rather than through these intervening variables. However, there were significant negative indirect effects of gasoline prices on miles driven via number of cars owned and via income. The latter indirect effect is made up of three components. The first is that each are percent increase in gasoline prices was associated with a .6 percent decrease in income which resulted in a .16 percent decrease in miles driven as a direct result of the lowered income. The second component of the indirect effect of gasoline prices on miles driven via income operates through vehicle ownership. Each one percent increase in gasoline prices was associated with a .6 decrease in income. This, in conjunction with the fact that each one percent increase in income was associated with a .13 percent increase in number of cars owned and with the fact that miles driven was associated positively with number of cars owned (each one percent increase in number of cars owned increased miles driven by .3 percent), implies that this indirect effect is equal to -.024. The third component of the indirect effect of gasoline prices through income operated through market orientation which was also highly income elastic and had a positive impact on miles driven. As was the case with the component of the indirect effect of gasoline prices on miles driven via income which operated through vehicle ownership this third component of the indirect effect was trivial when compared to the total effect of gasoline prices on income. Each one percent increase in gasoline prices has the effect via income and market orientation of decreasing miles driven by a meager (.014) percent.

The combination of two facts about this analysis suggests that the type of driving which is responsive to gasoline prices is unrelated to the trip to work and other normal household activities such as shopping and transporting children to school. These are that: 1) the structural relationships presented in Figure 11.3 incorporate most of the components of variance in work-related driving (logarithm of miles to work, and logarithm of weeks worked) as well as factors which should positively effect the amount of household related driving (number of adults and number of children) and the length of local driving circuits (logarithm city size, logarithm distance to center of city) and 2) the direct effect of gasoline price on miles driven overwhelms any indirect effects via these components. Since nearly one-third of the miles driven nationally in passenger vehicles were for social and recreational purposes, even moderate reductions in this sort of driving would be sufficient to explain the responsiveness observed cross-sectionally. Prior to the oil embargo of 1973, nearly 15 percent of miles driven were designated as related to pleasure rides.[3] Such pleasure rides are likely to be quite responsive to gasoline prices.

In order to assess the extent to which individual households are able to adapt to changes in gasoline prices (rather than the extent to which the population as a whole has adapted to intra-areal price differences, to which the coefficients in Table 11.6 pertain), a system similar to that presented in Figure 11.3 was estimated for change in the logarithm of miles driven between 1973 and 1977 for those households owning a vehicle in both years. Because five years is not a long enough period for major residential changes or changes in public transportation systems to occur, the change analysis differs from the 1973 cross-sectional analysis in that public transportation, city size, distance to center of city, and miles to work are considered to be at the same causal level as change in gasoline price. This is not to say that these variables are not affected by changes in gasoline prices, but rather that the system is not likely to have reached an equilibrium at this point in time. Hence, we choose not to assert that the observed association of changing gasoline price and, for instance, the existence of public transportation within walking distance is necessarily causal.

Because of the very disruptive nature of the recent increases in gasoline prices, we allow for the possibility in our estimation procedures that the strength of relationships between variables in addition to the value of independent variables have changed over the period. We allow for this by including both initial level and change measures in the regression equations when the structural relationships are estimated. So long as the error term behaves in accordance with classical assumptions the coefficient on initial level in these regression equations can be interpreted as the amount of change in the strength of the relationship while the coefficient on the change measures represents the importance of change in the independent variable in determining the dependent variables.[4] If the system had attained equilibrium by the end of the period, therefore, we would expect the coefficient on the change variables to equal the coefficients obtained in the cross-sectional analysis. Algebraically,

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[3] See Oak Ridge National Laboratory (1978).

[4] A sufficient condition for the combined error term ($e_{77}-e_{73}$) to be normally distributed with mean zero and be orthogonal to the independent variables is that they are separately normally distributed with mean zero and orthogonal to the independent variables.

$$\begin{array}{l} \ln(\text{Miles}_{73}) = A_{73} + b_{73} \ln(GP_{73}) + e_{73} \\ \underline{\ln(\text{Miles}_{77}) = A_{77} + b_{77} \ln(GP_{77}) + e_{77}} \\ \ln(\frac{\text{Miles}_{73}}{\text{Miles}_{77}}) = A_{73} - A_{77} + b_{73} \ln(GP_{73}) - b_{77} \ln(GP_{77}) + e_{73} - e_{77} \end{array}$$

Adding and subtracting the term $b_{77} \ln(GP_{73})$ from the righthand side of this equation and rearranging terms, we see:

$$(2) \quad \text{Ln}(\frac{\text{Miles}_{73}}{\text{Miles}_{77}}) = A_{77} - A_{73} + b_{77} [\ln(GP_{77}) - \ln(GP_{73})] + (b_{77} - b_{73}) \ln GP_{73} + (e_{77} - e_{73}).$$

A further modification incorporated in the model is that a measure of post-embargo mobility is included in the change equations both additively and interactively with prices. The rationale for including these measures in the estimation equation is that people who had moved since the rapid increase in prices experienced in the first quarter of 1974 would have been more likely than nonmovers to adapt to the new price levels. Table 11.7 presents direct and total effects of the various independent variables on change in miles driven between 1973 and 1977, along with their standard errors. The first column of coefficients refers to the direct effect of change in the independent variable on change in miles driven, while the second column indicates the degree of change in association $(b_{77} - b_{73})$ which had occurred over the period. The third column represents the total effect of change in the independent variable on change in miles driven, and the fourth column indicates the total change in coefficients. Because market orientation and the logarithm of number of cars owned immediately precede the change in logarithm of miles driven in the logical ordering set forth in Figure 11.3, their total effects and direct effects are equal. Longitudinally, each one percent increase in market orientation that is measured by the logarithm of dollars spent eating out had the effect of increasing the difference in logarithm miles driven in 1977 and logarithm miles driven in 1973 by .0289 percent. This coefficient is highly significant. Over the same period, the strength of the relationship between market orientation and miles driven increased from the 1973 level by slightly more than .03 percent.

The most important variables in explaining change in miles driven over the period were changes in the level of vehicle ownership, income, family structure, and place of residence. In the case of income and number of adults, there was

Table 11.7

DIRECT AND TOTAL EFFECTS OF FACTORS INFLUENCING CHANGE IN MILES DRIVEN
[Dependent Variable: ln(Miles77)-ln(Miles 73)]

	Direct Effects of		Total Effects of	
	Change in Variable	Initial Level	Change in Variable	Initial Level
Market Orientation	0.0289** (0.0090)	0.0307** (0.0092)	.0289** (.0090)	.0307** (.0092)
ln(Number of Cars Owned)	0.2326** (0.0654)	0.0853 (0.0763)	.2326** (.0654)	.0853 (.0763)
ln(Income)	0.1701** (0.0459)	0.0264 (0.0409)	.2266** (.0445)	.0932* (.0363)
Whether Public Transit	0.0291 (0.0437)	0.0942 (0.0462)	0.0261 (0.0440)	0.0906* (0.0462)
ln(Miles to Work)	0.0193 (0.0167)	-0.0094 (0.0180)	0.0329 (0.0167)	-0.0028 (0.0179)
ln(City Size)	-0.0151 (0.0167)	0.0072 (0.0117)	-0.0094 (0.0165)	0.0105 (0.0116)
ln(Weeks Worked)	0.0118 (0.0112)	0.0113 (0.0170)	0.0209 (0.0112)	0.0295+ (0.0162)
ln(Weeks Worked, Spouse)	0.0185 (0.0151)	0.0018 (0.0115)	0.0366* (0.0147)	0.0058 (0.0114)
ln(Miles to City)	-0.0102 (0.0140)	0.0508 (0.0206)	-0.0068 (0.0141)	0.0489 (0.0207)
ln(Number of Adults)	0.2223** (0.0658)	0.1268* (0.0632)	0.3751** (0.0598)	0.1680** (0.0575)
ln(Number of Children)	-0.0543 (0.0468)	0.0475 (0.0359)	-0.0625 (0.0470)	0.0358 (0.0356)
Age	-0- (-0-)	-0.0008 (0.0016)	-0- (-0-)	-0.0020 (0.0016)
ln(Gasoline Price)	1.9050+ (1.0176)	0.6455 (0.4576)	1.3747 (1.0124)	0.2931 (0.4535)
Whether Moved	2.0036** (0.6090)	-0- (-0-)	1.7965* (0.6102)	-0- (-0-)
Moved Price Interaction	-4.1456** (1.2270)	-0- (-0-)	-3.7423** (1.2301)	-0- (-0-)

$N = 3232$
$R^2 = .0502$

also a significant and powerful change in coefficients over the five year period. Not only did income changes have a powerful positive effect on change in miles driven (each one percent increase in income being associated with a .23 percent increase in miles driven), but this effect seems to have become more powerful over time (having increased by .09 during the period). Similarly, each one percent change in number of adults in the household had the effect of increasing the change in miles driven by approximately .4 percent, and this represented an increase in the strength of the relationship of .17 percent.

Perhaps the most interesting result indicated by the coefficients of Table 11.7 is that for nonmovers there was an apparent positive, though insignificant, price elasticity for gasoline, whereas for movers there was a strong negative price elasticity of miles driven. Interestingly enough, the price elasticity for those people who changed residence sometime during the period from 1973 through 1977 was almost identical to the long-run elasticity identified from the 1973 cross-section. That is, for movers the price elasticity of -2.3676 (i.e., -3.7423 +1.3747, the sum of the price-mover interaction coefficient and the price coefficient) was less than one-half of a standard deviation away from the -2.28 elasticity found in the cross-section.[5]

As was the case in the cross-sectional analysis, longitudinal changes in gasoline prices affected changes in miles driven directly rather than via the intervening variables for the most part. There is, however, one rather interesting indirect effect of a price component operating through changes in income. The association between gasoline prices and income which was negative in 1973 became significantly and powerfully more negative over the period. One possible explanation of this finding is that the recession which followed the oil embargo of 1973-74 was more severe in areas where gasoline prices were already high. These areas are generally confined to the north central and northeastern United States. The rate of growth in these areas has over the past two decades been less than that in the southern and western regions of the country and, hence, part of the indirect effect of gasoline prices on miles driven via income may be spurious.

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[5] The size of this interaction term's coefficient raised the question of whether there were significant structural differences between movers and stayers with respect to other variables as well as the price variable. In order to test this notion the regressions were run separately for the two groups and a Chow test performed. While the two groups were significantly different in terms of the Chow test, almost all of the difference resulted from the price-mobility interaction--none of the other coefficients differed significantly or importantly across the two groups.

There are a number of other interesting, though less powerful, results indicated in Table 11.7. For instance, while there was no evidence that increased availability of public transit decreased miles driven, there was a significant change in the coefficient of public transportation on miles driven. The .0942 percent change in the direct coefficient indicates that the existence of public transportation within walking distance no longer reduced the number of miles driven as much as it did in the 1973 cross-section. This may be an indication of continued deterioration in the quality of public transportation relative to private automobile travel.

Another interesting, though not powerful, effect is that of the change in coefficient on distance to center of city. While people living further out did not increase their driving as much as people living close to the center of the city--a result which was similar to the cross-sectional finding, the .0489 coefficient indicates that this negative association of miles driven and distance to center of city was no longer as powerful as it had been in 1973.

Cross-sectional and longitudinal data show unequivocably that the most important component of the variation in the burden of gasoline expenditure is variation in the number of miles driven. The variation in gasoline prices observed across areas and through time was so trivial in comparison to the variation in miles driven from household to household or from year to year that its effect on the burden of gasoline prices was negligible. The impact of variations in income was approximately half as important as variation in expenditure.

While from an accounting point of view, variation in gasoline prices is not important in accounting for variation in the burden of gasoline expenditures, from a behavioral point of view it is. When we restricted our attention to households that remained reasonably stable over the 1973-77 period and who owned and operated vehicles in each of those years, we found that changes in gasoline prices were a significant and important predictor of changes in miles driven. The regression results from the 1973 cross-sectional analysis of these households indicated a long-run price elasticity slightly greater than -2. This elasticity was of the same general size as that found in cross-national analyses. Longitudinally, where change in miles driven was analyzed on the basis of change in gasoline prices, we found price elasticities approximately one half the size of the long-run elasticity. This apparent responsiveness, however, is not the same for all people. Specifically, we found that households who moved over the period in which the change measures were observed to exhibited a price responsiveness equal to the long run price elasticity found from the 1973 cross-sectional analysis, while those who did not move showed an insignificant positive price elasticity.

In the third and final section of this report, we expand upon these findings by asking the question: "Are there any identifiable subgroups of population who are much more heavily burdened by increased gasoline prices than average households?"

ISOLATION OF SEVERELY BURDENED SUBGROUPS

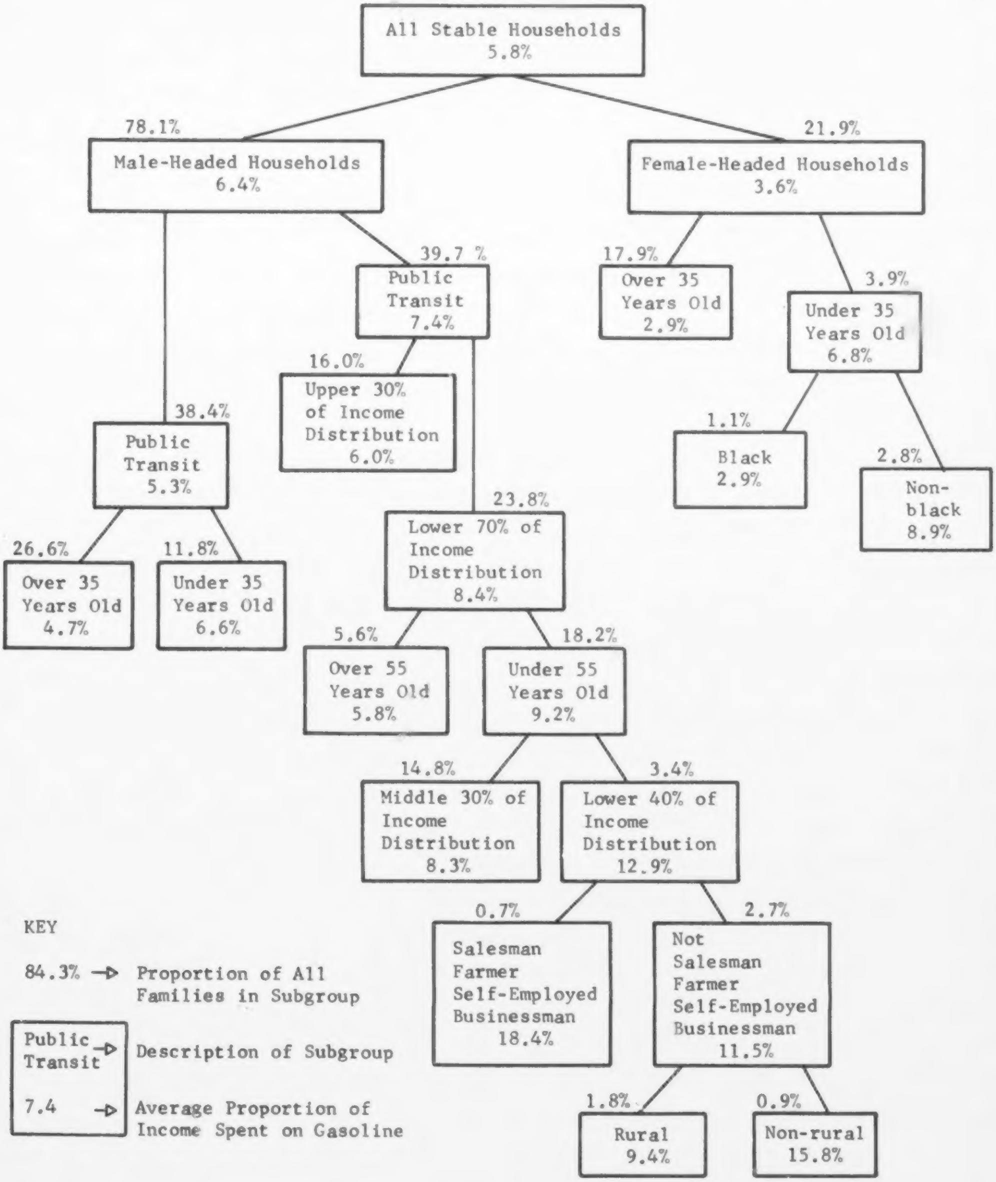
While the relationships estimated in the preceding section indicate considerable behavioral response to increased gasoline prices both in the short and intermediate run, the adaptation process takes time to work itself out. During this time certain types of households may be more heavily burdened than others. Holmes (1975) found that over a one-year period male headed households living a considerable distance from the center of a large city had significantly higher burdens than other households. Because income and the number of miles driven may vary substantially from year to year, we re-investigated the burden of gasoline miles on various types of families using five year average family income and estimated gasoline expenditures over a five year period (1973-1977).

We employed the algorithm SEARCH (the most recent version of AID) in conducting this investigation. This algorithm proceeds by searching among all the predictive variables to find that variable which maximally explains burden differences when the cases are dichotomized according to its values. Once such a split has been performed, the process is repeated for each of the resulting subgroups. The process continues until none of the predictor variables can explain as much as .8 percent of the variance in the dependent variable with any of the subgroups on that predictor. Figure 11.4 shows the actual splits which occurred when five-year average gasoline expenditure as a proportion of five-year average income was analyzed with a set of predictors intended to isolate possible heavily burdened subgroups. These predictors describe characteristics of the household head (age, race, sex, occupation, education), of the family (total family income), and of the residential location (whether public transportation within walking distance, region, city size, and distance to center of city).

The most powerful predictor in terms of ability to reduce variance for the entire population was the sex of the head of the household. Overall, 5.8 percent

Figure 11.4

FIVE-YEAR AVERAGE ESTIMATED GASOLINE EXPENDITURE AS A PROPORTION OF FIVE-YEAR AVERAGE INCOME
(SEARCH analysis of all stable families.)


All Stable Households
5.8%
78.1%
Male-Headed Households
6.4%
21.9%
Female-Headed Households
3.6%
39.7 %
Public Transit
7.4%
17.9%
Over 35 Years Old
2.9%
3.9%
Under 35 Years Old
6.8%
16.0%
Upper 30% of Income Distribution
6.0%
38.4%
Public Transit
5.3%
1.1%
Black
2.9%
2.8%
Non-black
8.9%
23.8%
Lower 70% of Income Distribution
8.4%
26.6%
Over 35 Years Old
4.7%
11.8%
Under 35 Years Old
6.6%
5.6%
Over 55 Years Old
5.8%
18.2%
Under 55 Years Old
9.2%
14.8%
Middle 30% of Income Distribution
8.3%
3.4%
Lower 40% of Income Distribution
12.9%
0.7%
Salesman Farmer Self-Employed Businessman
18.4%
2.7%
Not Salesman Farmer Self-Employed Businessman
11.5%
1.8%
Rural
9.4%
0.9%
Non-rural
15.8%
KEY
84.3% → Proportion of All Families in Subgroup
Public Transit → Description of Subgroup
7.4 → Average Proportion of Income Spent on Gasoline

of family money income over the five-year period was expended on gasoline. For the 987 female heads of households in the sample it was only 3.6 percent, a figure which is only 56 percent that of the average gasoline expenditure for male headed households (6.4 percent). When one looks only at female-headed households, the most powerful predictor is age. Women under 35 years of age spent as much as male-headed households whereas older female heads spent a mere 2.9 percent of their income over the five-year period on gasoline. For the younger women, the most important predictor was race, with blacks on average spending 1.6 percent on gasoline whereas nonblacks spent an average of nearly 9 percent. The policy relevance of these findings is clear. The national energy policy of allowing gasoline prices to rise, on average, has a less severe impact on female-headed households than on male-headed households, but certain types of female headed households, specifically households headed by young-nonblack women, are quite heavily burdened. A more in depth investigation of characteristics of households headed by young black women showed that 89 percent of these households lived within walking distance of public transportation and only 9 percent owned a vehicle. In contrast, 83 percent of the households headed by young nonblack woman owned a vehicle and only 58 percent lived within walking distance of public transportation. Households headed by older females of both races had low average gasoline expenditure burdens because of relatively low vehicle ownership rates (51.5 percent) and because of their proximity to public transportation (56 percent).

While age is an important determinant of the burden of five-year average gasoline expenditure for males, the existence of public transportation within walking distance is more powerful. Approximately one-half of the households headed by males lived within walking distance of public transportation and had an average expenditure burden of 5.3 percent, as opposed to an average expenditure of 7.4 percent for those households headed by males without public transportation nearby. Only for those male household heads with no public transportation available was age the most important predictor of the expenditure burden. Those less than 35 years of age spent nearly two percentage points more (6.6 percent vs. 4.7 percent) than their older counterparts. For male headed households not living within walking distance of public transportation, income (which is definitionally related to the dependent variable) was the most powerful predictor of expenditure burden. The SEARCH algorithm splits this group on income with the break point coming at about $20,000. Those earning less than this amount expended on average 8.4 percent of their income on gasoline as opposed to 6 percent for those with higher incomes.

The SEARCH algorithm splits the low-income male household heads who did not have access to public transportation into groups with higher and lower burdens on the basis of age, income, occupation, and urbanicity. However, it is fair to say that with the exception of men who are older than 55 years of age, the cost of gasoline was a substantial burden to all households. This was especially true for those in the bottom four deciles of family income, whose average five-year gasoline expenditure was nearly 13 percent of their total family income. And if the household happened to be headed by a farmer, salesman, or self-employed businessman, the gasoline expenditure averaged more than 18 percent. Some of this apparent burden, of course, is probably related to business expenses and can, under existing law, be written off as business expense. Nevertheless, even those who were not employed in these categories spent an average of almost 12 percent of their incomes on gasoline, with those in nonrural areas averaging nearly 16 percent. The modal household in this latter group was headed by a white married southern male with two children, one car, and a 50 percent chance of being unemployed as of the time of the 1978 interview.

Even when we look at a five year span instead of, as Holmes (1975) did, at a one-year period, these basic conclusions still hold: by and large, a policy based on increased gasoline prices will have a progressive impact, but it will hurt certain small subgroups of the population inordinately. A policy could be formulated to alleviate the excessive burden faced by a small minority.

SUMMARY

In preceding sections we have examined the relative burden of gasoline prices both cross-sectionally for 1977 and longitudinally for the 1973 through 1977 period. In addition we have examined the issue of how adaptable American consumers are to increased gasoline prices. Generally, the increased burden of gasoline prices has fallen on those best able to pay. There are, however, some subgroups on the population--notably households headed by young males who did not have access to public transportation--who are expending a disproportionate amount on gasoline. Since a key element of the national energy plan is to allow petroleum prices to increase, the policymakers should be aware of the existence of a minority of the population which will be disproportionately affected.

In analyzing the adaptability of households to changes in the gasoline prices, long-run and intermediate-run price elasticities were calculated.

Long-run price elasticity based on the 1973 cross-sectional response to variations in gasoline prices was found to be just over -2. This indicates considerable adaptability. The intermediate, or five-year, price elasticity was found to be roughly half that magnitude. For families who had changed residential location over the five-year period, however, the price elasticity was virtually identical to the long run price elasticity of -2. This means that people can and do adapt, apparently in response to changes in gasoline prices, but that this adaptation takes time. The growth in total miles driven, and therefore in petroleum demand, has resulted primarily from growth of the number of households (8.5 percent since 1973) and growth in real income. With the exception of 1973-1974 observations income grew over this period at a faster rate than gasoline prices rose. Thus, gasoline prices as a percentage of family income declined after the initial doubling of prices experienced in early 1974.

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Chapter 12

SUMMARY OF OTHER RESEARCH

Cramer, James C. Employment Trends of Young Mothers and the Opportunity Cost of Babies in the United States.

Cramer, James C. Fertility and Family in the United States.

Daymont, Thomas N. Racial Equity, Racial Equality, and Reverse Discrimination.

Espenshade, Thomas J. The Economic Consequences of Divorce.

Feinberg, Robert M. Market Structure and Employment Instability.

Gramlich, Edward M. and Wolkoff, Michael J. A Procedure for Evaluating Income Distribution Policies.

Hamermesh, Daniel. Entitlement Effects, Unemployment Insurance and Employment Decisions.

Hamermesh, Daniel. New Estimates of the Incidence of the Payroll Tax.

Harris, Richard J. The Rewards of Migration for Social Status and Social Mobility: 1968 - 1973.

Hill, Martha S. The Wage Effects of Marital Status and Children.

Hutchens, R.M. Welfare, Remarriage, and Marital Search.

Kiefer, Nicholas M. Identifying Restrictions in Limited Information Analysis of the Schooling Coefficient in a Wage Regression.

Kiker, B. F. and Condon, C. M. The Influence of Socioeconomic Background on the Earnings of Young Men.

Kim, Joochul. Why and Where People Move: Moving Decisions and Directionality in Current Migration Analysis.

Knaub, Norman L. The Impact of Food Stamps and Cash Welfare.

Lee, Lung-Fei. Estimations of a Model Choice Model for the Work Journey with Incomplete Observations.

Lillard, Lee A. Cross Section Versus Time Series Effects of Local Market Conditions.

Moen, Phyllis. Family Impacts of the 1975 Recession: Duration of Unemployment.

Null, David G. Work in progress.

Salkever, David S. Effects of Children's Health on Maternal Hours of Work: A Preliminary Analysis.

Shorrocks, Anthony F. Income Instability in the United States.

Silberman, Jonathan and Yochum, Gilbert. Minorities, Home Purchase and Discrimination.

Silberman, Jonathan and Yochum, Gilbert. Minority Groups and Home Purchase: Estimates of Internal and External Discrimination.

EMPLOYMENT TRENDS OF YOUNG MOTHERS AND THE OPPORTUNITY COST OF BABIES IN THE UNITED STATES
James C. Cramer, University of California at Davis
(Published in Demography, May 1979, Volume 16, No. 2, 177-197)

The central concept of microeconomic theories of fertility is opportunity cost--the product of wife's employment lost due to childbearing and the value of her employment. This paper presents a model for analyzing opportunity cost using panel data. The average loss of employment attributable to a second- or higher-order birth, calculated at around age 2 from PSID data, is over 400 hours per year. This time cost represents an income loss of about $1050 in 1969 dollars. Time cost is independent of such demographic factors as birth order and age of oldest sibling. Neither does time cost depend on husband's wage rate or wife's education or potential wage rate. This indicates that many microeconomic models of fertility have been seriously misspecified. The paper also compares results from static and dynamic models, explores possible problems due to simultaneity bias, investigates the relationship between changes in employment (including time cost) and initial employment level, and identifies the difficulties of theorizing about opportunity cost.

FERTILITY AND FAMILY IN THE UNITED STATES
James C. Cramer, University of California at Davis
Final report to National Institute for Child Health and Development, August 1979.

We began our research by asking whether couples directly experience economic consequences of childbearing and whether these consequences affect decisions about family size, and we have found that both questions can be answered affirmatively. Children, especially when they are young, have large impacts on wife's employment and on family standard of living. Wife's employment, in turn, is related to subsequent fertility and expected family size. Other economic factors also are important, at least in some samples and models; both fertility and wife's employment often are predicted by husband's wage rate level and trend and by wife's potential wage rate in the directions expected from microeconomic theory and speculation. These relationships generally persist when paid child care and other mediating factors are controlled. We conclude that couples do indeed tend to make decisions according to their ability to afford children.

This conclusion must be qualified in several important respects, however. The effects of economic factors on fertility usually are weak and sometimes are inconsistent in different models and samples. The relationship between fertility and wife's employment is due to behavioral constraints and is not observed in couples' stated plans and expectations. Husband's and wife's desires for additional children are strongly associated with subsequent fertility and expected family size, but both are unrelated to wife's actual or expected employment. Similarly, employment plans are unrelated to subsequent fertility and expected family size. Thus the relationship between fertility and employment is not the result of an overall life's plan, but rather it seems to evolve as a consequence of actual experiences. A final reservation is that sex roles ideologies and attitudes appear also to be related to both fertility and wife's employment. Because we have quite poor measures of sex roles, and because the unobserved sex roles variables probably are correlated with economic factors, our results may be biased--effects due to sex roles attitudes may be attributed falsely here to wage rates or wife's employment.

At every step, our research has shown the importance of methodological issues. Results often are different depending on how income is measured and whether wife's employment (as an independent variable) is dichotomized or measured as annual hours. Nonrecursive models often give unstable results. Considerable differences often are found between static and dynamic models and between actual and intended behavior. Many of our hypotheses have required nonlinear and nonadditive models, and estimates from these models often have been unstable between samples (even when estimates appear to be significant in one sample). Because of data limitations we often have had to look for small effects with unreliable models and small samples. We have proceeded carefully and are quite gratified with the results reported here, but inevitably our final conclusion is that further research is needed on the topics examined here.

RACIAL EQUITY, RACIAL EQUALITY, AND REVERSE DISCRIMINATION

Thomas N. Daymont, Ohio State University

This paper represents an attempt to improve our understanding of how changes in racial dimensions of labor market processes will influence future levels of racial inequality in earnings. To do this, PSID data are used in a simulation approach to project future levels of the black-white earnings ratio under different assumptions about labor market and educational institutions. The more interesting projections are based on the assumption of racial equity in labor market and educational institutions. The pattern of these future indicators based on this assumption suggest that there would be a significant initial reduction in racial inequality and an asymptotic convergence toward equality. However, this convergence would not be very fast: starting from an initial value of .75 in 1977, it would take 16 years for the ratio to reach .84 and almost 50 years for it to reach .95. Thus, even if labor market and educational attainment processes were to no longer discriminate on the basis of race, the unfavorable backgrounds of blacks would result in lower levels of economic attainment for blacks than whites well into the next century.

THE ECONOMIC CONSEQUENCES OF DIVORCE

Thomas J. Espenshade, Florida State University
Published in Journal of Marriage and the Family, August 1979 615-625.

In order to evaluate properly the rising incidence of divorce in the United States we need to know its consequences for families and society at large. This paper focuses on the questions: (1) what does divorce mean in the way of economic hardship for family members; (2) how is the burden distributed among husbands, wives, and children; and (3) why does the economic well-being of divorced wives typically fall short of that of former husbands? The analysis is based mainly on the findings from the PSID and we conclude that an explanation revolves around lost "economies of scale," the selective nature of divorce, work versus welfare, and the inadequacy of child support and alimony.

MARKET STRUCTURE AND EMPLOYMENT INSTABILITY

Robert M. Feinberg, Pennsylvania State University
(Published in Review of Economics and Statistics, 61 (November 1979) 497-505.)

Using three alternative measures of employment instability at the worker level, this paper examines the effects of worker characteristics and of market concentration of employers. Even after controlling for worker characteristics, more concentrated industries provide less stability of employment, except for women and workers at the highest education levels. The question whether higher wages compensate for this instability is left open.

A PROCEDURE FOR EVALUATING INCOME DISTRIBUTION POLICIES
Edward M. Gramlich, Michael J. Wolkoff, University of Michigan
(Published in The Journal of Human Resources, Volume XIV, #3, pp. 319-350.)

The paper develops a procedure for evaluating the social benefits of income redistribution programs. The procedure makes social benefit a function of both the mean and the variance of family income and needs of the recipients over time, with sensitivity tests to see how benefits change under various assumptions about donor preferences. Costs include budget costs and any gains or losses in producer or consumer surplus. The procedure is applied to three income redistribution programs: negative income taxes, public employment, and minimum wages. PSID data are used to simulate the income needs paths of 2000 families with and without the programs and compare the social benefits and costs for the three programs.

ENTITLEMENT EFFECTS, UNEMPLOYMENT INSURANCE AND EMPLOYMENT DECISIONS
Daniel Hamermesh, Michigan State University
(Published in the Economic Inquiry, Volume XVII, July 1979.)

Many studies have identified the work disincentive effects of unemployment insurance. This paper points out the entitlement effect of this and other social insurance programs on employment, as the risks of labor-market participation are reduced by the existence of unemployment benefits. This employment-increasing effect can offset the disincentive effect, with the net impact on employment becoming an empirical issue. Using data from the PSID on a sample of married women for 1971, we find that on net there is a slight negative effect on employment of higher unemployment insurance benefits. The entitlement effect on employment is, though, generally positive and significant, suggesting that the provisions of social insurance induce increased labor-force participation among women who otherwise would remain out of the labor force.

NEW ESTIMATES OF THE INCIDENCE OF THE PAYROLL TAX
Daniel Hamermesh, Michigan State University
(Published in Southern Economic Journal, April 1979, 45 (4).)

This study takes a new approach to empirical examination of tax incidence, basing the empirical work on equations describing individuals' earnings. The data are for 1973 from the Panel Study of Income Dynamics; they cover 587 male workers; and the presence of data for these men for 1967-1972 enables us to estimate the time path of shifting. The shifting process is fairly rapid; nearly 80 percent of that part of the tax that is shifted is done within one year of its imposition. The results show that little more than one-third of the employer's

OASDHI contribution is shifted back onto labor. The rest is either passed on to consumers or borne entirely by owners of capital.

THE REWARDS OF MIGRATION FOR SOCIAL STATUS AND SOCIAL MOBILITY: 1968-1973

Richard J. Harris, University of Texas, at San Antonio
Paper presented at meeting of the American Sociological Association, Boston, September 1979.

Integrating aspects of the "human capital" and "status attainment" models of individual achievement, this study specifically focuses on the relationship between migration and the attainment of occupational and income levels. The general model advanced for this analysis is that community structure and individual factors predetermine to a large extent the possibilities for individual attainment. Differential access to rewards and privileges is determined by the relative importance of ascribed, particularistic and achieved bases of attainment. Migration is viewed as an intervening mechanism for altering the particularistic dimension, alleviating at least one aspect of social inequality--the inequality of opportunity associated with residing in one type of community rather than another. The data used in this analysis are obtained from the first seven years of the Panel Study of Income Dynamics. The eligible sample is restricted to 1763 male heads of household in the labor force, 20 to 57 years of age in 1968, who remain in the sample as heads of household through 1974. Descriptive comparisons are presented examining the relationships between income change, occupational mobility and different patterns of migration. Finally, multiple regression with categorical dummy independent variables is used to assess the simultaneous effects of change in opportunity structure through migration and migrant selectivity in the model of status attainment. Considerable income mobility is observed, with net gains for both those encountering upward and downward occupational mobility. The traditional indicator or opportunity structure--community size--does not provide an adequate framework to evaluate the benefits of migration in this recent time period. In fact, migration from smaller to larger communities is negatively related to the attainment of both occupational status and income levels, independent of differential migrant selectivity. Between 1968 and 1973, the principle benefits of migration are found in income attainment rather than occupational mobility. Migrants between areas with different wage structures and levels of unemployment attained higher incomes than comparable non-migrants. These findings stand in contrast to the results of previous research and may indicate a change in the nature of "opportunity structures" in the United States.

THE WAGE EFFECTS OF MARITAL STATUS AND CHILDREN

Martha S. Hill, Institute for Social Research, University of Michigan
(Published in The Journal of Human Resources, Vol. 24, No. 4, Fall 1979.)

Using data from the ninth wave of the Panel Study of Income Dynamics, changes in the wage effects of marital status and number of children for workers of the same race and sex are analyzed as more refined measures of work experience, training, and labor force attachment are substituted for conventional measures of these factors. The results indicate that number of children is a good proxy variable for differential work history and labor market attachment among white women, and that marital status is not a proxy for such differences among any of the four major race/sex subgroups of workers, including white women. Overall, the findings suggest that, controlling for numerous aspects of worker qualifications, workers with greater financial responsibilities to their families receive higher wages.

WELFARE, REMARRIAGE, AND MARITAL SEARCH

Robert M. Hutchens, Cornell University
(Published in American Economic Review, 69 (June, 1979) 369-379.)

Logit regression estimates "are consistent with the hypothesis that AFDC transfers reduce the probability of remarriage." Using sample members in twenty states with large AFDC populations, and estimating the guarantee and the break-even income levels in each state, the "natural experiment" here relies on the assumption that it is the AFDC parameters that account for interstate differences in remarriage rates between 1970 and 1972. A 10% increase in AFDC will reduce remarriages by 8%.

IDENTIFYING RESTRICTIONS IN LIMITED INFORMATION ANALYSIS OF THE SCHOOLING COEFFICIENT IN A WAGE REGRESSION

Nicholas M. Kiefer
(C.O.R.E. Paper #7916, May 1979, Louvain-la-Neuve, Belgium)

Identifying restrictions underlying limited information estimates of the coefficients of a wage equation are considered from a Bayesian point of view. Within this framework "exclusion" restrictions need not be imposed exactly, and it becomes possible to consider the marginal densities of interesting coefficients as functions of the tightness of these restrictions. in the application considered here using PSID data, the two-stage least squares estimate of a schooling coefficient is higher than the ordinary least squares estimate, a finding common in the recent literature. As the exclusion restrictions are relaxed, however, the posterior mean for this coefficient falls to well below the OLS estimate.

The paper also serves as an example of the feasibility of Bayesian limited information analysis of a current economic issue.

THE INFLUENCE OF SOCIOECONOMIC BACKGROUND ON THE EARNINGS OF YOUNG MEN

B.F. Kiker, University of South Carolina
C.M. Condon, East Carolina University

In an effort to examine the effect of socioeconomic background on earnings of young men, a multi-equation recursive model of the economic achievement process is specified and then estimated using a national sample of young men. The data applied to the model contain the variables used by others in studies of earnings determination as well as achievement motivation, parent's income, and father's IQ, which are widely believed to be important in understanding the achievement process, but have not been available generally to other analysts. The analysis provides insight into a number of important issues. (1) Socioeconomic background differences are significant factors, directly and indirectly, in explaining earnings differences. The measured influence of socioeconomic background, however, is dependent on the variable used to specify it. (2) It appears that achievement motivation is an important determinant of individual earnings and, like educational attainment, acts as a channel for the integenerational transmission of economic inequality. (3) After controlling for differences in experience and occupation, neither IQ nor race are significant determinants of earnings.

THE IMPACT OF FOOD STAMPS AND CASH WELFARE ON FOOD EXPENDITURES, 1971-1975

Norman L. Knaub, Pennsylvania State University

The 1971 amendments to the Food Stamp Program reduced the purchase requirements for most food stamp recipients and completely eliminated the purchase requirements for the poorest households. This study utilized the Panel Study Income Dynamics (PSID) data for the period 1971-1975 in order to assess the impact of this program alteration on the food expenditures of participants. The sample for this study contained 3,388 households of which between 714 to 830 (depending on the year) households were recipients of food stamps and the remainder were food stamp nonparticipants with comparable income relative to family size.

A multiple regression analysis was run for the food expenditures of food stamp participants and nonparticipants. The results indicated that during 1971-1975 the Food Stamp Program somewhat restrained the recipients so that the transfer was utilized for additional expenditures on food for home consumption. When the income of food stamp recipients was disaggregated, the marginal propensity to expend (MPE) on food at home was, on the average, five times as large as the MPE's from cash welfare and nonwelfare income. However, the MPE on food from bonus food stamps indicated that between \$.43 to \$.67 per additional \$1.00 of bonus food stamps was diverted to nonfood purchases. A similar study by this author for 1968-1970 indicated that the entire value of bonus food stamps was utilized for food purchases. Consequently, during 1971-1975, the Food Stamp Program accomplished a major program objective by stimulating additional food expenditures, but the degree of success in this areas has been reduced by the 1971 amendments to the Food Stamp Act.

WHY AND WHERE PEOPLE MOVE: MOVING DECISIONS AND DIRECTIONALITY IN CURRENT MIGRATION ANALYSIS
Joochul Kim
(Ph.D. dissertation for degree awarded October 1979)

Data from the PSID are used to better understand important questions of "why and where" people moved during the time period between 1969 and 1977. For the decision-making process of household migration, this study adapted the intervening variable theory utilizing the "index of aspiration-ambition". Empirical results indicated that neither the "index of aspiration-ambition" nor traditional status variables are adequate in explaining the household decision-making process of migration. However, an alternative approach suggested that dynamic changes of household characteristics, particularly noneconomic related changes, explain a substantial portion of total variance in the estimated equation. While there was no single pattern of effectiveness in economic well-being among different types of migrants, nonmigrants seemed to sustain somewhat more improvements than migrants in their economic well-being.

Based on five types of migration direction created by the usage of the percent urban population of each county, there was clear evidence that the direction of migration is toward the most ruralward migration. Furthermore, while the most ruralward migration direction was particularly prevailing in the Northeast, the direction of migration seemed to be heavily toward the most urbanward migration in the South.

ESTIMATIONS OF A MODEL CHOICE MODEL FOR THE WORK JOURNEY WITH INCOMPLETE OBSERVATIONS
Lung-Fei Lee, University of Minnesota
Discussion Paper No. 77-86, June 1977

In this paper, we have developed a five equation model for the travel to work journey for the population in the U.S. economy and estimated it with PSID data. The data on cost and time are incomplete; they are available only for the chosen mode but not for the alternatives. In this problem, we are interested in the worker's choices of driving own car or using public carriers for the work journey. We have applied some two stage consistent procedures and a two step maximum likelihood procedure to handle these incomplete data problems.

We have found that personal characteristics such a sex and race play an important role in choosing the travel modes. The environments, costs and time consideration are also quite important. The value of time was found to be \$0.96 per hour which is 21 percent of the average wage rates in our samples. This value of time is compatible with previous studies on the modal choice for the work journeys. The private mode was found to be the less costly and much faster travel mode for the average individual in the population. The misleading impression that private mode is faster but much more expensive is caused mainly because the self-selectivity factors have not been treated or the incomplete data problem has not been handled properly.

CROSS SECTION VERSUS TIME SERIES EFFECTS OF LOCAL MARKET CONDITIONS

Lee A. Lillard, Rand Corporation

The objective of this research is to bring together into a single model some of the major facilities of both micro and macro labor market models through use of panel data. The study will provide evidence on the distribution of the effects of changes in labor market conditions among workers, differences in the effects of cross section and time series variation in labor market conditions, and a micro model of work expectations about future economic conditions. This project requires the merging of several data sources and the capability to process large data files. The primary source of data is the PSID. National, state, SMSA and county data on local business conditions will be gathered from many sources and merged with the PSID panel data.

FAMILY IMPACT OF THE 1975 RECESSION: DURATION OF UNEMPLOYMENT

Phyllis Moen, Cornell University

(Published in Journal of Marriage and the Family, August, 1979 Volume 41, 561-571.)

Using the PSID data, this study examines the likelihood of extended joblessness for breadwinners who were unemployed during the recession of 1975. Both husband-wife and female-headed families with children under 18 are included in the analysis. Using log-linear techniques, several factors are found to increase the probability of unemployment lasting 15 weeks or more. A contextual variable, the county unemployment rate, is negatively related to extended joblessness; the "hard-core" unemployed have trouble getting reemployed even under favorable economic conditions. Women who head families are more likely to be unemployed for an extended period than are male breadwinners. Family heads with young children (under six) are more likely to be unemployed 15 weeks or more than are breadwinners of families at later stages of the life cycle. Policy considerations to lessen the potential for prolonged joblessness of breadwinners are discussed.

WORK IN PROGRESS
David G. Null, California State Polytechnic University
Paper presented at April 1979 meetings of the Pacific Sociological Society

This research involves the testing of a development of the "Wisconsin model of socioeconomic achievement" on the educational achievement of Black and White males who are under 25 in the 1973 wave. The data set has been recoded so that the subjects are matched with their parental families in a single case so that true intergeneration analysis can be done. The following variables are in the model (in reverse order of their closeness to the independent variable: educational achievement): race, parental nAch, parental I.Q., parental education, parental occupation, parental desires for their offspring's education, number of offspring, offspring nAch, offspring I.Q., offspring educational achievement.

EFFECTS OF CHILDREN'S HEALTH ON MATERNAL HOURS OF WORK: A Preliminary Analysis
David S. Salkever, Johns Hopkins University

This paper investigates the importance of the health status of children on the labor supply of the mother. A structural labor supply model with market wage and shadow wage equations is modified to account for the health status of the children. The model is estimated with PSID data and shows weak support for the model, with greater labor supply effects estimated for one parent families.

INCOME INSTABILITY IN THE UNITED STATES
Anthony F. Shorrocks, University of Wisconsin
Discussion Paper, Institute for Research on Poverty, December, 1978.

This study applies a new index of income mobility which exploits the fact that changes in relative income positions over time are reflected in the degree to which incomes are equalized as the accounting period is extended. Using the PSID data, stability profiles are constructed for different age, sex, occupation, and education groups, and the extent to which the level and type of income mobility varies across these groups is discussed. The paper also compares the stability of earnings with that of total family income, and examines the degree to which the index values are sensitive to the choice of the measure of inequality.

MINORITIES, HOME PURCHASE AND DISCRIMINATION

Jonathan Silberman and Gilbert Yochum, Old Dominion University

Recent studies have found that proportional differences in homeownership between minorities (blacks, Spanish, and females) and whites are due to supply restrictions in the housing market. This interpretation of proportional differences between groups leaves out the possibility that individuals within the affected group act according to their preferences for homeownership. Considering only the effect of external stimuli on individual behavior may not reflect an accurate assessment of the source and extent of discrimination with respect to particular outcomes. Our approach has been to define the components of discrimination using a behavorial framework. In addition to external supply constraints, proportional differences in homeownership among groups may be the result of choices that reflect the habits and customs peculiar to a minority, especially habits and customs keyed to past discrimination.

Using the PSID data, we find that proportional differences in homeownership among minority headed households do result from market supply restrictions but more importantly, are the result of the current behavioral effects of past discrimination. From a policy perspective our approach suggests that a program of services and counseling (information about housing markets) be implemented to alter internal behavior patterns by moving to dispell the effects of past discrimination on current behavior.

MINORITY GROUPS AND HOME PURCHASE; ESTIMATES OF INTERNAL AND EXTERNAL DISCRIMINATION

Jonathan Silberman and Gilbert Yochum, Old Dominion University
(Paper prepared for presentation at the annual meetings of the Southern Economic Association November 7-9, 1979, Atlanta, Georgia)

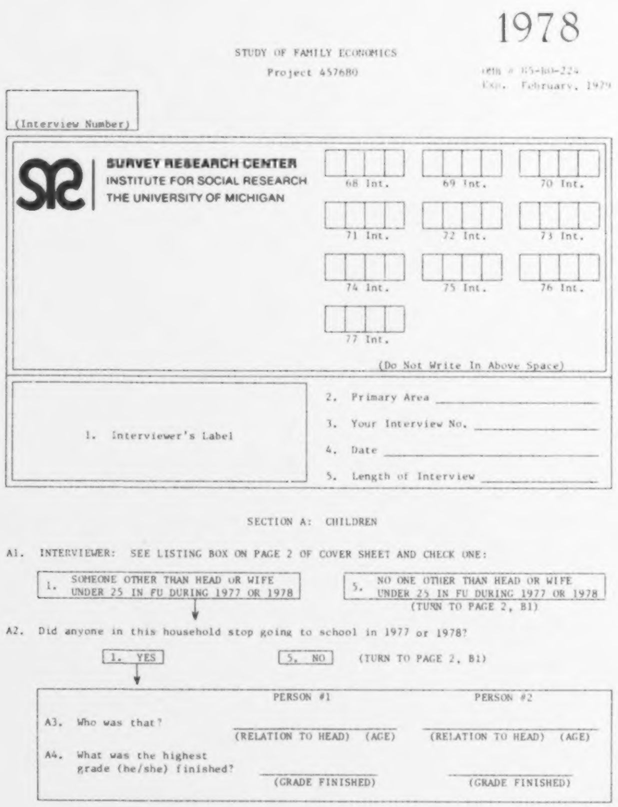
The objective of this current paper is to approach the issue of discrimination in housing markets from a different perspective than previous contributions. It is our thesis that proportional homeownership differences among whites and minorities may be the result not only of overt attempts by one group to exclude another from certain facets of social or economic life, but also may result from choices made by the latter that reflect the habits, customs, traditions, and values unique to individuals within that group. Such an interpretation implies that households within the affected group may act differently.

Our approach results in an interpretation of discrimination in housing markets that can be linked to a behavorial model. Discrimination is delineated into two components: the result of external stimuli; and the result of internal characteristics. Hypotheses regarding the extent and movement of discrimination in housing markets will be formulated based upon the behavorial model. The empirical test uses the PSID.

Appendix A

1978 QUESTIONNAIRE

Although the questionnaire, codes, and study procedures are described each year in a separate documentation volume, we reproduce the 1978 questionnaire in this appendix for readers without access to these volumes.



1978

STUDY OF FAMILY ECONOMICS
Project 457680

OMB # 85-R0-224
Exp. February, 1979

(Interview Number)

SURVEY RESEARCH CENTER
INSTITUTE FOR SOCIAL RESEARCH
THE UNIVERSITY OF MICHIGAN

68 Int. 69 Int. 70 Int.
71 Int. 72 Int. 73 Int.
74 Int. 75 Int. 76 Int.
77 Int.

(Do Not Write In Above Space)

1. Interviewer's Label

2. Primary Area ____________

3. Your Interview No. ____________

4. Date ____________

5. Length of Interview ____________

SECTION A: CHILDREN

A1. INTERVIEWER: SEE LISTING BOX ON PAGE 2 OF COVER SHEET AND CHECK ONE:

1. SOMEONE OTHER THAN HEAD OR WIFE UNDER 25 IN FU DURING 1977 OR 1978

5. NO ONE OTHER THAN HEAD OR WIFE UNDER 25 IN FU DURING 1977 OR 1978 (TURN TO PAGE 2, B1)

A2. Did anyone in this household stop going to school in 1977 or 1978?

1. YES 5. NO (TURN TO PAGE 2, B1)

	PERSON #1	PERSON #2
A3. Who was that?	(RELATION TO HEAD) (AGE)	(RELATION TO HEAD) (AGE)
A4. What was the highest grade (he/she) finished?	(GRADE FINISHED)	(GRADE FINISHED)

SECTION B: TRANSPORTATION

(ASK EVERYONE)

B1. Is there public transportation within walking distance of (here) (your house)?

1. YES   5. NO (GO TO B3)

B2. Is it good enough so that a person could use it to get to work?

____

____

B3. Do you (or anyone else in the family here) own a car or truck?

1. YES   5. NO (TURN TO PAGE 3, C1)

B4. How many cars and trucks do you (and your family living here) own?

____

B5. During the last year how many miles did you (and your family) drive in (your car/all of your cars)?

____

____

(TURN TO PAGE 3, C1)

SECTION C: HOUSING

C1. How many rooms do you have for your family (not counting bathrooms)? __________

C2. Do you live in a one-family house, a two-family house, an apartment, a mobile home, or what?

1. ONE-FAMILY   3. APARTMENT   7. OTHER (SPECIFY) __________
2. TWO-FAMILY   4. MOBILE HOME

C3. Do you own the (home/apartment), pay rent, or what?

1. OWNS OR IS BUYING   3. PAYS RENT (TURN TO PAGE 4, C11)   8. NEITHER OWNS NOR RENTS (TURN TO PAGE 4, C16)

(IF OWNS OR IS BUYING)

C4. About how much did all your utilities like heat and electricity cost you last year?

$__________ PER YEAR

C5. Could you tell me what the present value of your house (farm) is--I mean about what would it bring if you sold it today?

$__________

C6. Do you have a mortgage on this property?

YES   NO (TURN TO PAGE 5, C20)

	1st MORTGAGE	2nd MORTGAGE
C7. About how much is the remaining principal on this mortgage?	$______	$______
C8. How much are your monthly mortgage payments?	$______	$______
C9. About how many more years will you have to pay on it?	______	______

C10. Do you also have a second mortgage?

YES (RETURN TO C7)   NO (TURN TO PAGE 5, C20)

(TURN TO PAGE 5, C20)

(IF PAYS RENT)

C11. About how much rent do you pay a month? $__________

C12. Is this (house/apartment) rented furnished?

1. YES 5. NO

C13. Do you pay for any of the utilities like heat and electricity yourself?

1. YES 5. NO (TURN TO PAGE 5, C20)

C14. About how much did they cost you altogether last year?

$ __________ PER YEAR

C15. Is heating included in your monthly rent?

1. YES 5. NO

(TURN TO PAGE 5, C20)

(IF NEITHER OWNS NOR RENTS)

C16. How is that? __________

C17. How much would it rent for if it were rented? $__________ PER __________ (MONTH, YEAR)

C18. Do you pay for any of the utilities like heat and electricity yourself?

YES NO (TURN TO PAGE 5, C20)

C19. About how much did they cost you altogether last year?

$__________ PER YEAR

(TURN TO PAGE 5, C20)

(ASK EVERYONE)

C20. Have you (HEAD) moved any time since the spring of 1977?

1. YES 5. NO (GO TO C23)

C21. What month was that? __________ (MOST RECENT MOVE)

C22. Why did you move? __________

C23. Do you think you might move in the next couple of years?

1. YES; MIGHT OR MAYBE 5. NO 8. DON'T KNOW (TURN TO PAGE 6, D1)

C24. Would you say you definitely will move, probably will move, or are you more uncertain?

1. DEFINITELY 2. PROBABLY 3. MORE UNCERTAIN

C25. Why might you move? __________

(TURN TO PAGE 6, D1)

SECTION D: EMPLOYMENT

D1. We would like to know about what you do--are you (HEAD) working now, looking for work, retired, a student, (a housewife), or what?

1. WORKING NOW
2. ONLY TEMPORARILY LAID OFF
3. LOOKING FOR WORK, UNEMPLOYED (TURN TO PAGE 15, E1)
4. RETIRED
5. PERMANENTLY DISABLED
6. HOUSEWIFE
7. STUDENT

→ TURN TO PAGE 19, F1

8. OTHER ________ (SPECIFY) →(GO TO D2 IF HAS JOB, OTHERWISE TURN TO PAGE 19, F1)

D2. What is your main occupation? (What sort of work do you do?)

____

____

____

OCC ☐☐ IND ☐☐

(IF NOT CLEAR)
D3. Tell me a little more about what you do.

____

____

D4. What kind of business is that in? ____

____

D5. Do you work for someone else, yourself, or what?

1. SOMEONE ELSE
2. BOTH SOMEONE ELSE AND SELF
3. SELF ONLY (GO TO D12)

D6. Do you work for the federal, state, or local government?

1. YES 5. NO

D7. Is your current job covered by a union contract?

1. YES 5. NO (GO TO D12)

D8. Do you belong to that labor union?

1. YES 5. NO (GO TO D12)

D9. When you work for others, do you work for the federal, state or local government?

1. YES 5. NO

D10. Is your current job covered by a union contract?

1. YES 5. NO (GO TO D12)

D11. Do you belong to that labor union?

1. YES 5. NO (GO TO D12)

D12. How much formal education is required to get a job like yours?

____

D13. Do you also have to have some work experience or special training to get a job like yours?

YES NO (GO TO D15)

D14. What kind of experience or special training is that?

____

D15. On a job like yours, how long would it take the average new person to become fully trained and qualified?

________ (YEARS) OR ________ (MONTHS) ☐☐☐

D16. When a job like yours becomes available, would there be many qualified people ready and eager to get it, very few, or what?

1. MANY
5. VERY FEW
7. OTHER (SPECIFY) ____

D17. Would a woman have a harder time getting a job like yours than a man, or an easier time, or what?

1. HARDER TIME
3. SAME
5. EASIER TIME
7. OTHER (SPECIFY) ____

D18. How long have you had your present position? ____ (MONTHS) OR ____ (YEARS) ☐☐☐

IF LESS THAN ONE YEAR ↓   IF ONE YEAR OR MORE → (GO TO D24)

D19. What month did you start this job? ____________

D20. What happened to the job you had before--did the company go out of business, were you laid off, promoted, were you not working, or what?

____________ [5. NO PREVIOUS JOB] (GO TO D24)

____

D21. On the whole, would you say your present job is better or worse than the one you had before?

[1. BETTER] ↓   [5. WORSE] ↓   [3. SAME] (GO TO D23)

D22. Why is it (better/worse)? ____________

____

____

D23. Does your present job pay more than the one you had before?

[1. YES, MORE]   [5. NO, SAME OR LESS]

(GO TO D24)

D24. Did you miss any work in 1977 because someone else in the family was sick?

[1. YES] ↓   [5. NO] (GO TO D26)

D25. How much work did you miss? ____ (DAYS) ____ (WEEKS) ____ (MONTHS)

D26. Did you miss any work in 1977 because you were sick?

[1. YES] ↓   [5. NO] (GO TO D28)

D27. How much work did you miss? ____ (DAYS) ____ (WEEKS) ____ (MONTHS)

D28. Did you take any vacation or time off during 1977?

[1. YES] ↓   [5. NO] (TURN TO PAGE 9, D30)

D29. How much vacation or time off did you take? ____ (DAYS) ____ (WEEKS) ____ (MONTHS)

D30. Did you miss any work in 1977 because you were on strike?

[1. YES] ↓   [5. NO] (GO TO D32)

D31. How much work did you miss? ____ (DAYS) ____ (WEEKS) ____ (MONTHS)

D32. Did you miss any work in 1977 because you were unemployed or temporarily laid off?

[1. YES] ↓   [5. NO] (GO TO D34)

D33. How much work did you miss? ____ (DAYS) ____ (WEEKS) ____ (MONTHS)

D34. Then, how many weeks did you actually work on your main job in 1977?

____________ WEEKS

D35. And, on the average, how many hours a week did you work on your main job in 1977?

____________ HOURS/WEEK

D36. Did you work any overtime which isn't included in that?

[1. YES] ↓   [5. NO] (TURN TO PAGE 10, D38)

D37. How many hours did that overtime amount to in 1977?

____________ HOURS

D38. Are you salaried, paid by the hour, or what?

1. SALARIED

D39. How much is your salary?

$________ PER ________

D40. If you were to work more hours than usual during some week, would you get paid for those extra hours of work?

1. YES   5. NO (GO TO D46)

D41. About how much would you make per hour for that overtime?

$__________ PER HOUR

(GO TO D46)

3. PAID BY HOUR

D42. What is your hourly wage rate for your regular work time?

$__________ PER HOUR

D43. What is your hourly wage rate for overtime?

$__________ PER HOUR

(GO TO D46)

7. OTHER

D44. How is that?

____

____

____

____

____

D45. If you worked an extra hour, how much would you earn for that hour?

$__________

(GO TO D46)

D46. Did you have any extra jobs or other ways of making money in addition to your main job in 1977?

1. YES   5. NO (TURN TO PAGE 11, D52)

OCC ☐☐

D47. What did you do? ______________________________

____

D48. Anything else? ______________________________

____

D49. About how much did you make per hour at this? $________ PER HOUR

D50. And how many weeks did you work on your extra job(s) in 1977? ________ (WEEKS)

D51. On tne average, how many hours a week did you work on your extra job(s)?

______________ HOURS PER WEEK

D52. Was there more work available (on your job/any of your jobs) so that you could have worked more if you had wanted to?

1. YES

D53. How much would you have earned per hour?

$________ PER HOUR →

5. NO OR DON'T KNOW

D54. Would you have liked to work more if you could have found more work?

1. YES (GO TO D57)   5. NO

D55. Could you have worked less if you had wanted to?

1. YES (GO TO D57)   5. NO

D56. Would you have preferred to work less even if you had earned less money?

1. YES   5. NO

(GO TO D57)

D57. About how much time does it take you to get to work each day, door to door?

____________ ONE WAY   00. NONE (TURN TO PAGE 12, D60)

D58. About how many miles is it to where you work? ____________ ONE WAY

D59. Do you use public transportation to get to work, drive with your wife, have a car pool, drive by yourself, walk, or what?

1. PUBLIC TRANSPORTATION   2. DRIVE WITH WIFE   3. CAR POOL   4. DRIVE BY SELF

5. WALK   7. OTHER (SPECIFY) ______________________

D60. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET AND CHECK ONE:

1. HEAD IS UNDER 45
1. HEAD IS 45-64 YEARS OLD (TURN TO PAGE 14, D83)
5. HEAD IS 65 OR OLDER (TURN TO PAGE 22, G1)

D61. We are interested in how people hear about and get their jobs. How old were you when you first got a job you thought of as a regular or possibly permanent job?

_______ AGE
98. HEAD DOES NOT HAVE AND NEVER HAD A REGULAR OR PERMANENT JOB (TURN TO PAGE 13, D72)

D62. What sort of work did you do on that job? ______________ ☐ OCC

D63. Was that the type of job that gave you a lot of useful skills or training?

1. YES
5. NO

D64. How did you first hear about that job--was it through a friend, a relative, a want ad, an employment agency or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR
2. RELATIVE
3. WANT AD
4. EMPLOYMENT AGENCY
7. OTHER (SPECIFY): ______________

D65. Do you think there was anyone who may have helped you get the job?

1. YES
5. NO (GO TO D71)

D66. Was that a friend, a relative, or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR
2. RELATIVE
7. OTHER (SPECIFY): ______________

D67. How did they help? ______________

D68. Did they work there?

1. YES
5. NO (GO TO D71)

D69. Could they have had some say in your getting the job?

1. YES
5. NO (GO TO D71)

D70. How much say do you think they had? ______________

(GO TO D71)

D71. Before you got the job, did you know anyone (else) who worked there?

1. YES
5. NO

D72. Are you still working for that same employer or are you now working for a different one?

1. SAME (TURN TO PAGE 22, G1)
5. DIFFERENT

D73. How long have you been working for your present employer?

_______ MONTHS OR _______ YEARS

D74. How did you first hear about a job with your present employer--was it through a friend, a relative, a want ad, an employment agency, or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR
2. RELATIVE
3. WANT AD
4. EMPLOYMENT AGENCY
7. OTHER (SPECIFY): ______________

D75. Is this the type of job that gives you useful training or skills?

1. YES
5. NO

D76. Do you think there was anyone who may have helped you get the job?

1. YES
5. NO (GO TO D82)

D77. Was that a friend, relative, or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR
2. RELATIVE
7. OTHER (SPECIFY): ______________

D78. How did they help? ______________

D79. Did they work for your present employer?

1. YES
5. NO (GO TO D82)

D80. Could they have had some say in your getting the job?

1. YES
5. NO (GO TO D82)

D81. How much say do you think they had? ______________

D82. Before you got your first job with your present employer, did you know anyone (else) who worked there?

1. YES
5. NO

(TURN TO PAGE 22, G1)

(ASK IF HEAD IS 45-64 YEARS OLD)

D83. Now I have some questions about retirement and planning for the future. At what age do you think you will retire from the main work you are now doing?

______ AGE AT RETIREMENT   NEVER   DON'T KNOW
(GO TO D85)

D84. Do you think you will retire before you are 65?

1. YES   5. NO   8. DON'T KNOW; DEPENDS

D85. We are interested in the income people expect to have when they retire. Let's look ahead to the time when you have reached 65. Will you be eligible for Social Security payments then?

1. YES   5. NO   8. DON'T KNOW

D86. Will you be eligible for other retirement pensions when you are 65?

1. YES   5. NO   8. DON'T KNOW

D87. Will you (and your wife) have any other sources of income if you retire at 65, such as income from interest, dividends, rent or annuities, veterans' benefits, or from part-time work?

1. YES   5. NO   8. DON'T KNOW

D88. Taking everything into account, if you retired at 65 would you expect to have a retirement income that was not enough, just enough, more than enough, or what?

1. NOT ENOUGH   3. JUST ENOUGH   5. MORE THAN ENOUGH   7. OTHER (SPECIFY): ______   8. DON'T KNOW

D89. Do you expect to have a home of your own paid for before you are 65?

1. YES   5. NO   8. DON'T KNOW

(TURN TO PAGE 22, G1)

SECTION E: IF LOOKING FOR WORK, UNEMPLOYED IN D1

OCC ☐☐

E1. What kind of job are you looking for? ______________________

E2. How much would you expect to earn? $________ PER ________

E3. Will you have to get any training to qualify? ________________

E4. Have you been doing anything in the last four weeks to find a job?

1. YES   5. NO (GO TO E6)

E5. How many places have you been to in the last four weeks to find out about a job?

0. NONE   1. ONE   2. TWO   3. THREE   4. FOUR   5. FIVE OR MORE

E6. Are there some jobs around here you wouldn't take because of where these jobs are located?

1. YES   5. NO   8. DON'T KNOW

E7. Are there some jobs around here you wouldn't take because of the hours they want you to work?

1. YES   5. NO   8. DON'T KNOW

E8. Are there jobs available around here that just aren't worth taking?

1. YES   5. NO (GO TO E10)

E9. How much do they pay? $________ PER ________

E10. Would you be willing to move to another community if you could get a good job there?

1. YES, MAYBE, OR DEPENDS   5. NO

E11. How much would a job have to pay for you to be willing to move?

$________ PER ________

E12. Why is that? ________________

E13. How long have you been looking for work? ________________

E14. Have you ever had a job?

1. YES ↓ 5. NO (TURN TO PAGE 22, G1)

E15. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET AND CHECK ONE:

1. HEAD IS UNDER 45 ↓ 5. HEAD IS 45 OR OLDER (TURN TO PAGE 17, E27)

E16. We are interested in how people hear about and get their jobs. How old were you when you first got a job you thought of as a regular or possibly permanent job?

______ AGE ↓ 98. HEAD HAS NEVER HAD A REGULAR OR PERMANENT JOB (TURN TO PAGE 17, E27)

E17. What sort of work did you do on that job? ______ ☐ OCC

E18. Was that the type of job that gave you a lot of useful skills or training?

1. YES 5. NO

E19. How did you first hear about that job--was it through a friend, a relative, a want ad, an employment agency or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR 2. RELATIVE 3. WANT AD 4. EMPLOYMENT AGENCY 7. OTHER (SPECIFY): ______

E20. Do you think there was anyone who may have helped you get the job?

1. YES ↓ 5. NO (GO TO E26)

E21. Was that a friend, a relative, or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR 2. RELATIVE 7. OTHER (SPECIFY): ______

E22. How did they help? ______

E23. Did they work there?

1. YES ↓ 5. NO (GO TO E26)

E24. Could they have had some say in your getting the job?

1. YES ↓ 5. NO (GO TO E26)

E25. How much say do you think they had? ______

E26. Before you got the job, did you know anyone (else) who worked there?

1. YES 5. NO

E27. What sort of work did you do on your last job? (What was your occupation?)

______ OCC ☐☐ IND ☐☐

E28. What kind of business was that in? ______

E29. What happened to that job--did the company go out of business, were you laid off, or what? ______

E30. When did you last work? ______

IF 1977 OR 1978 ↓ IF BEFORE 1977 (TURN TO PAGE 22, G1)

E31. Did you take any vacation or time off during 1977?

1. YES ↓ 5. NO (GO TO E33)

E32. How much vacation or time off did you take? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

E33. Did you miss any work in 1977 because someone else in the family was sick?

1. YES ↓ 5. NO (GO TO E35)

E34. How much work did you miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

E35. Did you miss any work in 1977 because you were sick?

1. YES ↓ 5. NO (GO TO E37)

E36. How much work did you miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

E37. Did you miss any work in 1977 because you were on strike?

1. YES ↓ 5. NO (TURN TO PAGE 18, E39)

E38. How much work did you miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

E39. Did you miss any work in 1977 because you were unemployed or temporarily laid off?

1. YES   5. NO (GO TO E41)

E40. How much work did you miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

E41. Then, how many weeks did you actually work on your job in 1977?

______ WEEKS

E42. And, on average, how many hours a week did you work when you worked?

______ HOURS PER WEEK

E43. On your last job, how much time did it take you to get to work each day, door to door?

______ ONE WAY   00. NONE (TURN TO PAGE 22, G1)

E44. About how many miles was it to where you worked? ______ ONE WAY

E45. Did you use public transportation to get to work, drive with your wife, have a car pool, drive by yourself, walk, or what?

1. PUBLIC TRANSPORTATION   2. DRIVE WITH WIFE   3. CAR POOL   4. DRIVE BY SELF   5. WALK

7. OTHER (SPECIFY): ______

(TURN TO PAGE 22, G1)

SECTION F: RETIRED, HOUSEWIFE, STUDENT, PERMANENTLY DISABLED

F1. INTERVIEWER CHECKPOINT: REFER TO D1 AND CHECK ONE:

1. RETIRED   5. PERMANENTLY DISABLED, HOUSEWIFE, STUDENT OR OTHER (TURN TO PAGE 20, F15)

F2. In what year did you retire? ______

(IF LESS THAN 20 YEARS AGO)   (IF 20 OR MORE YEARS AGO) → (TURN TO PAGE 20, F15)

F3. How did you happen to retire when you did? ______

F4. Had you planned to retire then, or did you retire unexpectedly, or what?

1. PLANNED TO RETIRE (GO TO F6)   5. RETIRED UNEXPECTEDLY   7. OTHER (SPECIFY): ______ (GO TO F6)

F5. What happened to make you retire? ______

F6. Were you willing or even glad to retire, or did you only retire because you had to, or what?

1. WILLING/GLAD TO RETIRE   5. ONLY RETIRED BECAUSE HAD TO   7. OTHER (SPECIFY): ______

F7. Have you worked and earned any money since you retired?

1. YES   5. NO

F8. Do you do any volunteer work, without pay, for a church, charity, or somewhere else?

1. YES   5. NO (TURN TO PAGE 20, F10)

F9. What do you do? ______

F10. Is there some kind of paid work you would do if a job like that were available?

1. YES   5. NO (GO TO F12)

F11. What kind of work would that be? ____________________

F12. Considering income and expenses, are you living better than before you retired, about as well, not quite as well, much worse, or what?

1. BETTER   2. ABOUT AS WELL   3. NOT QUITE AS WELL   4. MUCH WORSE

7. OTHER (SPECIFY): ____________________

F13. Do you feel that you have enough to live comfortably?

1. YES   5. NO

F14. Generally speaking, how do you feel about your life since retirement? ____________________

F15. During the last year (1977), did you do any work for money?

1. YES   5. NO (TURN TO PAGE 21, F22)

F16. What kind of work did you do when you worked? (What was your occupation?) ____________________

F17. What kind of business was that in? ____________________

F18. How many weeks did you work last year? __________ WEEKS

F19. About how many hours a week did you work (when you worked)? __________ HOURS PER WEEK

F20. Are you still working?

1. YES (TURN TO PAGE 21, F22)   5. NO

F21. What happened to that job--did the company go out of business, were you laid off, or what?

____

F22. Are you thinking of getting (a/another) job in the future?

1. YES   5. NO (TURN TO PAGE 22, G1)

F23. When might that be? (How soon?) ____________________

F24. What kind of job do you have in mind? ____________________

F25. Would you have to get any training to qualify?

1. YES   5. NO

F26. Have you been doing anything in the last four weeks to find a job?

1. YES   5. NO (GO TO F28)

F27. How many places have you been to in the last four weeks to find out about a job?

0. NONE   1. ONE   2. TWO   3. THREE   4. FOUR   5. FIVE OR MORE

F28. Are there some jobs around here you wouldn't take because of where these jobs are located?

1. YES   5. NO   8. DON'T KNOW

F29. Are there some jobs around here you wouldn't take because of the hours they want you to work?

1. YES   5. NO   8. DON'T KNOW

F30. Are there jobs around here that just aren't worth taking?

1. YES   5. NO (TURN TO PAGE 22, G1)

F31. How much do they pay? $__________ PER __________

SECTION G: WIFE'S WORK, HOUSEWORK AND FOOD

(ASK EVERYONE)

G1. Are you married, widowed, divorced, separated, or single?

1. MARRIED (GO TO G4) | 2. SINGLE | 3. WIDOWED | 4. DIVORCED | 5. SEPARATED (GO TO G4)

G2. Were you ever married?

1. YES | 5. NO (GO TO G4)

G3. What happened to your last marriage--were you widowed, divorced, separated, or what?

3. WIDOWED | 4. DIVORCED | 5. SEPARATED | 7. OTHER (SPECIFY): ____________

G4. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET AND CHECK ONE:

1. MALE HEAD IS MARRIED WITH WIFE IN FU OR MALE HEAD HAS BEEN LIVING WITH FEMALE FRIEND FOR ONE YEAR OR MORE

5. ALL OTHERS (TURN TO PAGE 26, G40)

G5. Did your (wife/friend) do any work for money in 1977?

1. YES | 5. NO (TURN TO PAGE 24, G20)

OCC ☐☐ IND ☐☐

G6. What kind of work did she do? ____________

____

G7. What kind of business is that in? ____________

____

G8. Did your (wife/friend) miss any work in 1977 because someone else in the family was sick?

1. YES | 5. NO (GO TO G10)

G9. How much work did she miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

G10. Did your (wife/friend) miss any work in 1977 because she was sick?

1. YES | 5. NO (GO TO G12)

G11. How much work did she miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

G12. Did your (wife/friend) take any vacation or time off during 1977?

1. YES | 5. NO (GO TO G14)

G13. How much vacation or time off did she take? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

G14. Did your (wife/friend) miss any work in 1977 because she was on strike?

1. YES | 5. NO (GO TO G16)

G15. How much work did she miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

G16. Did your (wife/friend) miss any work in 1977 because she was unemployed or temporarily laid off?

1. YES | 5. NO (GO TO G18)

G17. How much work did she miss? ______ (DAYS) ______ (WEEKS) ______ (MONTHS)

G18. Then, how many weeks did she actually work on her main job in 1977? ______ (WEEKS)

G19. And, on the average, how many hours a week did she work on her main job in 1977?

____________ HOURS/WEEK

G20. Is your (wife/friend) working for money now?

1. YES — 5. NO (TURN TO PAGE 25, G39)

G21. About how much time does it take your (wife/friend) to get to work each day, door to door?

_______ ONE WAY — 00. NONE (GO TO G24)

G22. About how many miles is it to where she works? _______ ONE WAY

G23. Does she use public transportation to get to work, drive with you, have a car pool, drive by herself, walk, or what?

1. PUBLIC TRANSPORTATION — 2. DRIVE WITH HEAD — 3. CAR POOL — 4. DRIVE BY SELF — 5. WALK

7. OTHER (SPECIFY): _______

G24. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, PAGE 2 AND CHECK ONE:

1. WIFE/FRIEND IS UNDER 45 — 5. WIFE/FRIEND IS 45 OR OLDER (TURN TO PAGE 25, G34)

G25. How long has your (wife/friend) been working for her present employer?

_______ MONTHS OR _______ YEARS

G26. How did she first hear about a job with her present employer--was it through a friend, a relative, a want ad, an employment agency, or what?

1. FRIEND, ACQUAINTANCE, NEIGHBOR — 2. RELATIVE — 3. WANT AD — 4. EMPLOYMENT AGENCY

7. OTHER (SPECIFY): _______

G26a. Is this the type of job that gives her useful skills or training?

1. YES — 5. NO

G27. Was there anyone who may have helped her get that job?

1. YES — 5. NO — 8. DON'T KNOW (TURN TO PAGE 25, G33)

G28. Was that a friend, a relative or who?

1. FRIEND, ACQUAINTANCE, NEIGHBOR — 2. RELATIVE — 7. OTHER (SPECIFY): _______

G29. How did they help? _______

G30. Did they work there?

1. YES — 5. NO (GO TO G33)

G31. Could they have had some say in her getting the job?

1. YES — 5. NO — 8. DON'T KNOW (GO TO G33)

G32. How much say do you think they had? _______

G33. Before she got the job, did she know anyone (else) who worked there?

1. YES — 5. NO

G34. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, PAGE 2, AND CHECK ONE:

1. WIFE/FRIEND IS 45-64 — 5. WIFE/FRIEND IS UNDER 45 OR OVER 64 (GO TO G39)

G35. At what age do you think your (wife/friend) will retire from the main work she is now doing?

_______ AGE — NEVER — DON'T KNOW

G36. Do you think she will retire before she is 65?

1. YES — 5. NO — 8. DON'T KNOW; DEPENDS

G37. Will she be eligible for Social Security payments from her own work?

1. YES — 5. NO — 8. DON'T KNOW

G38. Will she be eligible for other retirement pensions of her own?

1. YES — 5. NO — 8. DON'T KNOW

G39. About how much time does your (wife/friend) spend on housework in an average week -- I mean time spent cooking, cleaning and doing other work around the house?

_______ (HOURS PER WEEK)

G40. About how much time do you (HEAD) spend on housework in an average week? (I mean time spent cooking, cleaning, and doing other work around the house?)

__________ HOURS PER WEEK

G41. INTERVIEWER CHECKPOINT: CHECK ONE:

☐ PEOPLE OTHER THAN HEAD AND WIFE/FRIEND IN FU, INCLUDING CHILDREN

☐ 0. ONLY HEAD OR HEAD AND WIFE/FRIEND IN FU (TURN TO PAGE 27, G46)

G42. Does anyone else here in the household help with the housework?

☐ 1. YES ☐ 5. NO (TURN TO PAGE 27, G46)

	PERSON #1	PERSON #2	PERSON #3
G43. Who is that?	__________ (RELATIONSHIP TO HEAD) (AGE)	__________ (RELATIONSHIP TO HEAD) (AGE)	__________ (RELATIONSHIP TO HEAD) (AGE)
G44. About how much time does (he/she) spend on housework in an average week?	__________	__________	__________

G45. Anyone else? ☐ YES (ASK G43-45 ABOVE) ☐ NO (TURN TO PAGE 27, G46)

G46. Did you (or anyone else now living in your family) receive or buy government food stamps last month?

☐ YES ☐ NO (GO TO G55)

G47. For how many members of your family were stamps issued? __________

G48. How much did you pay for the stamps? $________ (AMOUNT) PER ________ (WEEK, MONTH)

G49. How much food could you buy with the stamps? $________ (AMOUNT) PER ________ (WEEK, MONTH)

G50. In addition to what you spent on food stamps, did you (FAMILY) spend any money on food that you use at home?

☐ YES ☐ NO (GO TO G52)

G51. How much? $________ (AMOUNT) PER ________ (WEEK, MONTH)

G52. Do you have any food delivered to the door which isn't included in that?

☐ YES ☐ NO (GO TO G54)

G53. How much do you spend on that food? $________ (AMOUNT) PER ________ (WEEK, MONTH)

G54. About how much do you (FAMILY) spend eating out, not counting meals at work or at school?

$________ (AMOUNT) PER ________ (WEEK, MONTH)

(TURN TO PAGE 28, G59)

G55. How much do you (FAMILY) spend on food that you use at home in an average week?

$________ (AMOUNT) PER ________ (WEEK, MONTH)

G56. Do you have any food delivered to the door which isn't included in that?

☐ YES ☐ NO (GO TO G58)

G57. How much do you spend on that food? $________ (AMOUNT) PER ________ (WEEK, MONTH)

G58. About how much do you (FAMILY) spend eating out, not counting meals at work or at school?

$________ (AMOUNT) PER ________ (WEEK, MONTH)

G59. Did you (FAMILY) use government food stamps (commodity stamps) at any time in 1977?

☐ 1. YES ☐ 5. NO (TURN TO PAGE 29, H1)

G60. How much did you pay for the stamps in 1977? $__________ (AMOUNT) PER __________ (WEEK, MONTH)

G61. How much food could you buy with the stamps in 1977? $__________ (AMOUNT) PER __________ (WEEK, MONTH)

G62. For how many months did you use food stamps in 1977? __________ (MONTHS)

SECTION H: INCOME

(ASK EVERYONE)

H1. To get an accurate financial picture of people all over the country, we need to know the income of all the families that we interview.

INTERVIEWER: CHECK ONE:

☐ 1. FARMER, OR RANCHER ☐ 5. NOT A FARMER OR RANCHER (GO TO H5)

H2. What were your total receipts from farming in 1977, including soil bank payments and commodity credit loans? $__________ A

H3. What were your total operating expenses, not counting living expenses? $__________ B

H4. That left you a net income from farming of? A - B = $__________ A-B

H5. Did you (R AND FAMILY) own a business at any time in 1977, or have a financial interest in any business enterprise?

☐ 1. YES ☐ 5. NO (GO TO H8)

H6. Was it a corporation or an unincorporated business, or did you have an interest in both kinds?

☐ 1. CORPORATION (GO TO H8) ☐ 2. UNINCORPORATED ☐ 3. BOTH ☐ 8. DON'T KNOW

H7. How much was your (FAMILY'S) share of the total income from the business in 1977--that is, the amount you took out plus any profit left in?

$__________

(ASK EVERYONE)

H8. How much did you (HEAD) receive from wages and salaries in 1977, that is, before anything was deducted for taxes or other things?

$__________

H9. In addition to this, did you have any income from bonuses, overtime or commissions?

YES NO (GO TO H11)

H10. How much was that? $____________

H11. Did you (HEAD) receive any other income in 1977 from:

(IF "YES" TO ANY ITEM, ASK "How much was it?" ENTER AMOUNT AT RIGHT)

(IF "NO" ENTER "0")

a)	professional practice or trade?	$______	PER ______
b)	farming or market gardening, roomers or boarders?	$______	PER ______
c)	dividends, interest, rent, trust funds, or royalties?	$______	PER ______
d)	ADC, AFDC?	$______	PER ______
e)	Supplemental Security Income (the gold/tan/yellow checks)?	$______	PER ______
f)	other welfare?	$______	PER ______

H12. INTERVIEWER CHECKPOINT: REFER TO H11d, H11e, and H11f AND CHECK ONE:

1. INCOME FROM ADC, AFDC, SUPPLEMENTAL SECURITY, OR OTHER WELFARE

5. NO SUCH INCOME (GO TO H16)

H13. Did welfare also help with your rent or other bills?

1. YES 5. NO (GO TO H15)

H14. What did they pay for? ____________

____

H15. There is a public program called Medicaid (Medi-Cal, Medical Assistance, Welfare, Medical Services) which provides medical assistance to persons in need. During the past year, has anyone in the family received medical care which has been or will be paid for by Medicaid (Medi-Cal, Medical Assistance, Welfare, Medical Services)?

1. YES 5. NO

H16. Did you (HEAD) receive any income in 1977 from Social Security?

YES NO (GO TO H18)

H17. How much was that? $__________ PER __________

H18. INTERVIEWER CHECKPOINT: REFER TO H16 AND CHECK ONE:

1. HEAD HAS INCOME FROM SOCIAL SECURITY

5. NO SUCH INCOME (TURN TO PAGE 31, H20)

H19. Do you have Medicare from Social Security? 1. YES 5. NO

(DO NOT WRITE IN THIS SPACE) HEAD TYPE INCOME:	
TAXABLE	TRANSFER
L A	☐☐

H20. Did you (HEAD) receive any income in 1977 from:

a)	other retirement pay, pensions, or annuities?	$______	PER ______
b)	unemployment compensation?	$______	PER ______
c)	workmen's compensation?	$______	PER ______
d)	alimony?	$______	PER ______
e)	child support?	$______	PER ______
f)	help from relatives?	$______	PER ______
g)	anything else? (SPECIFY) ______	$______	PER ______

H21. Did anyone (else) not living here now help you (FAMILY) out financially--I mean give you money, or help with your expenses during 1977?

YES NO (GO TO H23)

H22. How much did that amount to last year? $____________

H23. INTERVIEWER CHECKPOINT: DOES HEAD HAVE WIFE OR FEMALE FRIEND IN FU?

1. YES, WIFE/FRIEND IN FU

5. NO WIFE/FRIEND IN FU OR FU HAS FEMALE HEAD (TURN TO PAGE 32, H32)

H24. Did your (wife/friend) have any income during 1977?

1. YES 5. NO (TURN TO PAGE 32, H32)

H25. Was it income from wages, salary, a business, or what?

____________ SOURCE ____________ SOURCE

H26. How much was it before deductions?

$____________ $____________

H27. Did your (wife/friend) receive any unemployment compensation in 1977?

YES NO (GO TO H29)

H28. How much was that? ____________

H29. Did you (wife/friend) receive any Social Security in 1977?

1. YES 5. NO (TURN TO PAGE 32, H32)

H30. How much was that? $____________

H31. Does she have Medicare from Social Security? 1. YES 5. NO

(DO NOT WRITE IN THIS SPACE) WIFE TYPE INCOME:	
TAXABLE	TRANSFER
L A	☐☐

EXTRA EARNERS

H32. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, PAGE 2, ITEMS 1 AND 2, AND CHECK ONE:

☐ SOMEONE OTHER THAN CURRENT HEAD OR WIFE/FRIEND IN FU AT ANY TIME DURING 1977 ☐ NO SUCH PERSONS (TURN TO PAGE 34, H46)

H33. ☐ AT LEAST ONE OF THESE PERSONS IS OVER 13 ☐ NONE OF THESE PERSONS IS OVER 13 (TURN TO PAGE 34, H46)

LIST ALL SUCH PERSONS BY RELATION TO HEAD, INCLUDING A HEAD/WIFE WHO HAS MOVED OUT.

	____ (RELATION TO HEAD) ____ (AGE)	____ (RELATION TO HEAD) ____ (AGE)	____ (RELATION TO HEAD) ____ (AGE)	____ (RELATION TO HEAD) ____ (AGE)
H34. Did (MENTION PERSON) have any income in 1977?	☐ YES ☐ NO (GO TO H34 FOR NEXT PERSON LISTED)	☐ YES ☐ NO (GO TO H34 FOR NEXT PERSON LISTED)	☐ YES ☐ NO (GO TO H43 FOR NEXT PERSON LISTED)	☐ YES ☐ NO (GO TO H43 FOR NEXT PERSON LISTED)
H35. About how much did that amount to in 1977?	$____ IN 1977	$____ IN 1977	$____ IN 1977	$____ IN 1977
H36. Was that from wages, a pension, a business or what?	____ SOURCE TX ☐☐☐☐☐ ☐	____ SOURCE TX ☐☐☐☐☐ ☐	____ SOURCE TX ☐☐☐☐☐ ☐	____ SOURCE TX ☐☐☐☐☐ ☐
(IF WAGES OR BUSINESS)				
H37. What kind of work did (he/she) do?	____ OCCUPATION	____ OCCUPATION	____ OCCUPATION	____ OCCUPATION
H38. Can you tell me about how many weeks (he/she) worked?	____ WEEKS	____ WEEKS	____ WEEKS	____ WEEKS
H39. About how many hours a week was that?	____ HOURS	____ HOURS	____ HOURS	____ HOURS
H40. (IF DON'T KNOW) Was it more than half-time?	____	____	____	____
H41. Did (he/she) miss any work in 1977 because of unemployment or a strike? ☐ YES ☐ NO (GO TO H43)	☐☐☐☐ ☐	☐☐☐☐ ☐	☐☐☐☐ ☐	☐☐☐☐ ☐
H42. How much work did (he/she) miss?	(DAYS) (WEEKS) (MONTHS) ☐☐☐☐ ☐	(DAYS) (WEEKS) (MONTHS) ☐☐☐☐ ☐	(DAYS) (WEEKS) (MONTHS) ☐☐☐☐ ☐	(DAYS) (WEEKS) (MONTHS) ☐☐☐☐ ☐
H43. Did (he/she) have any other income?	☐ YES ☐ NO (GO TO H34 FOR NEXT PERSON LISTED	☐ YES ☐ NO (GO TO H34 FOR NEXT PERSON LISTED)	☐ YES ☐ NO (GO TO H34 FOR NEXT PERSON LISTED)	☐ YES ☐ NO (GO TO H43 FOR NEXT PERSON LISTED)
H44. What was that from?	____ SOURCE	____ SOURCE	____ SOURCE	____ SOURCE
H45. How much was that last year?	$____ IN 1977 TX ☐☐☐☐☐ ☐	$____ IN 1977 ☐☐☐☐☐ ☐	$____ IN 1977 ☐☐☐☐☐ ☐	____ IN 1977 ☐☐☐☐☐ ☐

(DO NOT WRITE IN THIS SPACE)

TAXABLE	TRANSFER	TAXABLE	TRANSFER	TAXABLE	TRANSFER	TAXABLE	TRANSFER
L A	☐☐	L A	☐☐	L A	☐☐	L A	☐☐

(ASK EVERYONE)

H46. Did anyone else living here in 1977 have any income? (INCLUDING CHILDREN UNDER 14)

[YES] ↓ [NO] (GO TO H48)

H47. Who was that?

__________ (RELATION TO HEAD) (AGE) __________ (RELATION TO HEAD) (AGE) __________ (RELATION TO HEAD) (AGE)

(TURN BACK AND ASK PAGE 32-33 FOR THESE ADDITIONAL MEMBERS)

H48. INTERVIEWER CHECKPOINT: REFER TO PAGE 32-33 AND CHECK ONE:

[1. EXTRA EARNER HAS ADC, AFDC, SUPPLEMENTAL SECURITY, OR OTHER WELFARE] ↓ [5. NO SUCH PERSONS] (GO TO H50)

H49. There is a public program called Medicaid (Medi-Cal, Medical Assistance, Welfare, Medical Services) which provides medical assistance to persons in need. During the past year, has anyone (else) in the family received medical care which has been or will be paid for by Medicaid (Medi-Cal, Medical Assistance, Welfare, Medical Services)?

[1. YES] [5. NO]

H50. INTERVIEWER CHECKPOINT: REFER TO PAGE 32-33 AND CHECK ONE:

[1. EXTRA EARNER HAS SOCIAL SECURITY] ↓ [5. NO SUCH PERSONS] (GO TO H52)

H51. Does anyone (else) in the family have Medicare from Social Security?

[1. YES] [5. NO]

H52. INTERVIEWER CHECKPOINT: CHECK ONE:

[1. FU INCLUDES PEOPLE OTHER THAN HEAD] ↓ [5. HEAD IS ONLY PERSON IN FU] (TURN TO PAGE 35, H57)

H53. Is anyone in the family looking for work?

[1. YES] ↓ [5. NO] (TURN TO PAGE 35, H57)

H54. Who is that? __________ (RELATION TO HEAD) (AGE) __________ (RELATION TO HEAD) (AGE)

H55. Has (he/she) been doing anything in the last four weeks to find a job? [1. YES] [5. NO] [1. YES] [5. NO]

H56. Anyone else? [YES] (GO BACK AND ASK H54-H55) [NO] (TURN TO PAGE 35, H57)

H57. Did you get any other money in 1977--like a big settlement from an insurance company, or an inheritance?

[1. YES] ↓ [5. NO] (GO TO H59)

H58. How much did that amount to? $__________ IN 1977

H59. Do you help support anyone who doesn't live here with you?

[1. YES] ↓ [5. NO] (GO TO H64)

H60. How many? __________

H61. How much money did that amount to in the last year? $__________ IN 1977

H62. Were any of these people dependent on you for more than half of their total support?

[1. YES] ↓ [5. NO] (GO TO H64)

H63. How many? __________

H64. Do you belong to a labor union?

[1. YES] [5. NO]

H65. Do you (HEAD) have any physical or nervous condition that limits the type of work or the amount of work you can do?

[1. YES] ↓ [5. NO] (TURN TO PAGE 36, H72)

H66. Does it limit your work a lot, somewhat, or just a little?

[1. A LOT] [3. SOMEWHAT] [5. JUST A LITTLE]

H67. How long have you had this condition?

__________ YEARS

H68. Do you expect it to get better, worse, or stay about the same?

[1. BETTER] [3. ABOUT THE SAME] [5. WORSE]

H69. Do you require a lot of extra care by someone?

[1. YES] ↓ [5. NO] (TURN TO PAGE 36, H72)

H70. Does that mean extra costs for the family?

[1. YES] ↓ [5. NO] (TURN TO PAGE 36, H72)

H71. Are those costs small, moderate, or quite large?

[1. SMALL] [3. MODERATE] [5. LARGE]

H72. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, PAGE 2, AND CHECK ONE:

☐ 1. FU INCLUDES PEOPLE OTHER THAN HEAD WHO ARE 18 OR OLDER

☐ 5. NO SUCH PEOPLE (TURN TO PAGE 37, H81)

H73. Is there anyone (else) 18 or older in this family who has any physical or nervous condition that limits the type of work or the amount of work they can do?

☐ 1. YES

☐ 5. NO (TURN TO PAGE 37, H81)

(ASK H74-80 FOR ALL SUCH PERSONS)

H74. Who is that?	(RELATION TO HEAD) (AGE)	(RELATION TO HEAD) (AGE)
H75. Does it limit their work a lot, somewhat, or just a little?	☐ 1. A LOT ☐ 3. SOMEWHAT ☐ 5. A LITTLE	☐ 1. A LOT ☐ 3. SOMEWHAT ☐ 5. A LITTLE
H75a. How long has (he/she) had this condition?	______ YEARS	______ YEARS
H76. Do you expect it to get better, get worse, or stay about the same?	☐ 1. BETTER ☐ 3. WORSE ☐ 5. SAME	☐ 1. BETTER ☐ 3. WORSE ☐ 5. SAME
H77. Does that person require a lot of extra care by someone?	☐ 1. YES ☐ 5. NO (GO TO H80)	☐ 1. YES ☐ 5. NO (GO TO H80)
(IF YES) H78. Does that mean extra costs for the family?	☐ 1. YES ☐ 5. NO (GO TO H80)	☐ 1. YES ☐ 5. NO (GO TO H80)
(IF YES) H79. Are those costs small, moderate, or quite large?	☐ 1. SMALL ☐ 3. MODERATE ☐ 5. LARGE	☐ 1. SMALL ☐ 3. MODERATE ☐ 5. LARGE

H80. Anyone else?

☐ 1. YES (ASK H74-79 ABOVE)

☐ 5. NO (TURN TO PAGE 37, H81)

H81. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, PAGE 2, AND CHECK ONE:

☐ 1. FU INCLUDES PEOPLE OTHER THAN HEAD WHO ARE 0-17 YEARS OLD

☐ 5. NO SUCH PEOPLE (TURN TO PAGE 38, J1)

H82. Is there anyone under 18 in this family who has any physical or nervous condition that limits their activity or schooling?

☐ 1. YES

☐ 5. NO (TURN TO PAGE 38, J1)

(ASK H83-89 FOR ALL SUCH PERSONS)

H83. Who is that?	(RELATION TO HEAD) (AGE)	(RELATION TO HEAD) (AGE)
H84. Does it limit their activity a lot, somewhat, or just a little?	☐ 1. A LOT ☐ 3. SOMEWHAT ☐ 5. A LITTLE	☐ 1. A LOT ☐ 3. SOMEWHAT ☐ 5. A LITTLE
H84a. How long has (he/she) has this condition?	______ YEARS	______ YEARS
H85. Do you expect it to get better, get worse, or stay about the same?	☐ 1. BETTER ☐ 3. WORSE ☐ 5. SAME	☐ 1. BETTER ☐ 3. WORSE ☐ 5. SAME
H86. Does that person require a lot of extra care by someone?	☐ 1. YES ☐ 5. NO (GO TO H89)	☐ 1. YES ☐ 5. NO (GO TO H89)
(IF YES) H87. Does that mean extra costs for the family?	☐ 1. YES ☐ 5. NO (GO TO H89)	☐ 1. YES ☐ 5. NO (GO TO H89)
(IF YES) H88. Are those costs small, moderate, or quite large?	☐ 1. SMALL ☐ 3. MODERATE ☐ 5. LARGE	☐ 1. SMALL ☐ 3. MODERATE ☐ 5. LARGE

H89. Anyone else?

☐ 1. YES (ASK H83-88 ABOVE)

☐ 5. NO (TURN TO PAGE 38, J1)

INTERVIEWER: REMEMBER TO FILL OUT "BY OBSERVATION" SECTION ON ALL INTERVIEWS.

SECTION J: NEW WIFE

J1. INTERVIEWER CHECKPOINT: REFER TO COVER SHEET, AND CHECK ONE:

☐ 1. FU HAS NEW WIFE/PERMANENT FRIEND THIS YEAR

☐ 5. FU HAS FEMALE HEAD

☐ 5. FU HAS SAME WIFE/PERMANENT FRIEND AS IN 1977 OR FU HAS NO WIFE/PERMANENT FRIEND

(TURN TO PAGE 39, K1)

J2. How many grades of school did your (wife/friend) finish?

GRADES OF SCHOOL

☐ 00 ☐ 01 ☐ 02 ☐ 03 ☐ 04 ☐ 05 ☐ 06 ☐ 07 ☐ 08 ☐ 09 ☐ 10 ☐ 11 ☐ 12

COLLEGE

☐ 13 ☐ 14 ☐ 15 ☐ 16 ☐ 17+

J3. Did she have any other schooling?

☐ 1. YES ☐ 5. NO (GO TO J8)

J4. What other schooling did she have?

____

____

____

____

(GO TO J8)

J5. What college was that?

____

J6. Does she have a college degree?

☐ 1. YES ☐ 5. NO (GO TO J8)

J7. Does she have any advanced degrees?

☐ 1. YES ☐ 5. NO

(GO TO J8)

J8. How much education did your (wife's/friend's) father have? __________

J9. How much education did your (wife's/friend's) mother have? __________

J10. How many years altogether has your (wife/friend) worked for money since she was 18?

______ YEARS ☐ 00. NONE (TURN TO PAGE 39, K1)

J11. How many of these years did she work full-time for most or all of the year?

______ YEARS ☐ ALL (TURN TO PAGE 39, K1)

J12. During the years that she was not working full-time how much of the time did she work?

____________________ ☐☐ ☐

(TURN TO PAGE 39, K1)

SECTION K: NEW HEAD

K1. INTERVIEWER: CHECK ONE:

☐ 1. FU HAS A NEW HEAD THIS YEAR

☐ 5. THIS FU HAS THE SAME HEAD AS IN 1977 (TURN TO PAGE 3 OF COVER SHEET)

K2. Now I have some questions about your family and past experiences. Where did your father and mother grow up? (FROM BIRTH TO 18 YEARS OF AGE.)

ST. CO- FA ☐☐☐☐☐ Father: __________ (STATE IF U.S., COUNTRY IF FOREIGN) __________ (COUNTY OR TOWN)

ST. CO- MO ☐☐☐☐☐ Mother: __________ (STATE IF U.S., COUNTRY IF FOREIGN) __________ (COUNTY OR TOWN)

K3. What was your father's usual occupation when you were growing up?

____

____________________ OCC ☐

K4. Thinking of your (HEAD'S) first full-time regular job, what did you do?

____________________ ☐ 0. NEVER WORKED (GO TO K6) OCC ☐

K5. Have you had a number of different kinds of jobs, or have you mostly worked in the same occupation you started in, or what?

____

____

K6. Do you (HEAD) have any children who don't live with you?

☐ YES ☐ NO (GO TO K9)

K7. How many? ______ NUMBER

1st ☐☐

2nd ☐☐

3rd ☐☐

K8. When were they born? ______ YEAR BORN ______ YEAR BORN ______ YEAR BORN

# ☐☐

K9. Did you (HEAD) have any children who are not now living?

# BY 25 ☐

☐ YES ☐ NO (TURN TO PAGE 40, K11)

K10. When were they born? ______ YEAR BORN ______ YEAR BORN ______ YEAR BORN

K11. How many brothers and sisters did you (HEAD) have? ____________ (SPECIFY NUMBER) 0. NONE (GO TO K13)

K12. Were any of your brothers or sisters older than you?

1. YES 5. NO

K13. Did you (HEAD) grow up on a farm, in a small town, in a large city, or what?

1. FARM 2. SMALL TOWN 3. LARGE CITY OTHER (SPECIFY): ____________

K14. In what state and county was that? (EXAMPLE: ILLINOIS, COOK COUNTY)

ST. CO- H ☐☐☐☐☐ ____________ STATE ____________ COUNTY

(IF DON'T KNOW TO K14) → K15. What was the name of the nearest town?

____________ TOWN

K16. What other states or countries have you lived in? (Including time spent abroad while in the armed forces.)

____

K17. Have you (HEAD) ever moved out of a community where you were living in order to take a job somewhere else?

1. YES (→ K19) 5. NO

K18. Have you ever turned down a job because you did not want to move?

1. YES 5. NO

K19. Were your parents poor when you were growing up, pretty well off, or what?

____

K20. How much education did your (HEAD'S) father have? ____________

(IF LESS THAN 6 GRADES) → K21. Could he read and write? ____________

K22. How much education did your (HEAD'S) mother have? ____________

(IF LESS THAN 6 GRADES) → K23. Could she read and write? ____________

K24. Are you (HEAD) a veteran?

1. YES 5. NO

K25. How many years have you (HEAD) worked since you were 18?

____________ YEARS 00. NONE (GO TO K28)

K26. How many of these years did you work full-time for most of the year?

____________ YEARS ALL (GO TO K28)

K27. During the years that you were not working full-time, how much of the time did you work?

____

____________ ☐☐ ☐

____

K28. How many grades of school did you (HEAD) finish?

GRADES OF SCHOOL: 00 01 02 03 04 05 06 | 07 08 09 10 11 12

COLLEGE: 13 14 15 16 17+

(00–06 →) K29. Did you get any other training?

1. YES 5. NO (GO TO K31)

K30. What was it?

____

____

____

K31. Do you have any trouble reading?

____

(07–12 →) K32. Did you have any other schooling?

1. YES 5. NO (GO TO K37)

K33. What other schooling did you have?

____

____

____

(13–17+ →) K34. What college was that?

____

K35. Do you have a college degree?

1. YES 5. NO (GO TO K37)

K36. Do you have any advanced degrees?

1. YES 5. NO

(TURN TO PAGE 3 OF COVER SHEET)

SECTION L: BY OBSERVATION ONLY

L1. Who was respondent (relation to Head)? ______________________

L2. Number of calls ______________________

L3. Is this address inside of the city limits of a city of 50,000 or more? (INTERVIEWER: CONSULT ROAD MAP IF NECESSARY.)

1. YES →

L4. What city is that? ______________________

L5. How far is this DU from the center of that city?

1. LESS THAN 5 MILES
2. 5 - 14.9 MILES
3. 15 - 29.9 MILES
4. 30 - 49.9 MILES
5. 50 OR MORE MILES

5. NO →

L6. What is the nearest city of 50,000 or more? ______________________

L7. How far is this DU from the center of that city?

1. LESS THAN 5 MILES
2. 5 - 14.9 MILES
3. 15 - 29.9 MILES
4. 30 - 49.9 MILES
5. 50 OR MORE MILES

L8. Is this address inside the city limits of a city of 5,000 or more?

1. YES 5. NO

THUMBNAIL SKETCH